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INEQUALITY IN LABOUR MARKETS

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“Mai i te kōpae ki te urupa, tātou ako tonu ai"

From the cradle to the grave we are forever learning

Declaration

This thesis is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the Preface and specified in the text.

It is not substantially the same as any that I have submitted, or, is being concurrently submitted for a degree or diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text. I further state that no substantial part of my dissertation has already been submitted, or, is being concurrently submitted for any such degree, diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text.

It does not exceed the prescribed limit of 60,000 words.

Athene Laws
October 2019

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Preface

The thesis contains three chapters, each of which studies a separate dimension of inequality in modern labour markets. Each chapter analyses the individual-level behaviours of workers or firms that underpin regional and aggregate distributional features of labour markets. Together, they address cross-sectional wage inequality policy at the national level, the propagation of labour market inequalities between regions, and the impacts on employment inequalities of long term structural changes in the labour market.

The first chapter considers the labour market propagation mechanisms of minimum wages, a policy commonly used to support low-wage workers. Extensive evaluations of minimum wages around the world typically find that higher minimum wages do not generate increased unemployment but are associated with substantial decreases in lower-tail wage inequality. The chapter provides the first empirical test of one explanation; a substantial search and labour supply response is behind the observed patterns.

The chapter identifies the impact of minimum wages on search, distinguishing the decision of whether to search (extensive margin) from the decision of how hard to search (intensive margin) for both non-working and working individuals. The analysis combines data on UK workers' search behaviour with quasi-experimental analysis of the UK minimum wage policy structure. Results find an increase in the number of individuals searching, but a decline in search intensity, and a corresponding increase in the duration of unemployed search. There is no evidence that workers already employed in low-wage jobs are discouraged from searching for higher paying jobs. The chapter shows that these results are consistent with search explanations of minimum wage labour market consequences.

The second chapter switches to addressing the spatial dimension of inequality, particularly the mechanisms that generate diverging outcomes between regions. The chapter models the individual firm employment responses to local shocks and the contributions these make to driving unequal employment rates between local regions.

The chapter builds a spatial network of the universe of UK firms with near pinpoint location accuracy and estimates the response of the local network to adverse employment events. Results show that firms in close proximity to a large mass layoff in turn reduce their own employment and that these negative spillovers are highly localised. The strength of the negative spillovers approximately halves for every kilometre further away from the event. The spillovers are also very persistent, with further localised employment losses continuing for at least five years after the event. In effect, a negative spiral is triggered at the local firm level, through a combination of sluggish individual firm adjustment and local agglomeration forces, and this can be used to explain the persistence in local labour market outcomes. The chapter also develops a new method for analysing spatial variation, and outlines the large costs associate with using more traditional techniques.

The third chapter, which is co-authored with Antonio Dias Da Silva and Filippos Petroulakis of the European Central Bank, is themed around the impact of long term, structural changes on employment inequality. Technological progress and deepening global integration have contributed to reduced middle-skill employment in a process commonly referred to as employment polarisation. Simultaneously, there has been a large decline in the number of hours worked per worker in European economies.

The chapter investigates the relationship between hours per worker and employment polarisation, asking whether hours per worker follow similar polarisation patterns. The analysis categorises occupations based on their task content, in particular the type and degree of routinisation involved. Results find large relative declines in hours per worker in routine manual jobs – precisely the occupations most negatively affected by employment polarisation from routine-biased technical change. A lower relative decline in hours per worker is observed in higher skilled jobs growing through polarisation. The patterns affect all age, gender and education groups approximately equally. The chapter concludes by evaluating the contribution of the hours per worker margin to overall employment polarisation.

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Chapter 1

Do minimum wages increase search effort?

Abstract

This paper identifies the impact of minimum wages on search, distinguishing the decision of whether to search (extensive margin) from the decision of how hard to search (intensive margin) for both non-working and working individuals. The analysis combines data on UK workers' search behaviour with quasi-experimental analysis of the UK minimum wage policy structure, including the 2016 introduction of the National Living Wage. I find an increase in the number of individuals searching, but a decline in search intensity, and a corresponding increase in the duration of unemployed search. Overall, there is no change in employment rates. The results are consistent with search explanations of minimum wage consequences. In contrast, no significant estimates are found for any on-the-job search moments, i.e. higher minimum wages do not provide a disincentive for workers to progress up job ladders.

Keywords: search, minimum wages, labour supply response

JEL codes: E24, J21, J64

1.1 Introduction

Large wage consequences concurrent with small employment consequences feature regularly in the minimum wage empirical literature. The pattern of impacts is often explained using search and matching theory yet direct evidence on search adjustment has been difficult to date. This paper is, I believe, the first to combine explicit evidence on search mechanisms and natural policy experiments to directly estimate the impact of minimum wages on multiple search margins. Using detailed data on search behaviour and minimum wage natural policy experiments, I separately identify the impact of minimum wages on extensive margin (participation) and intensive margin (effort) search for both employed and non-employed individuals. The analysis uncovers new stylised facts on the responsiveness of key labour market frictions to wage floor policies which are consistent with search theoretic explanations of minimum wage impacts.

Since the seminal work of Card and Krueger (1994) the majority of empirical findings on minimum wages have been inconsistent with the predictions of Walrasian labour market models. Minimal or zero adverse employment consequences are common microeconometric study results across diverse settings.[1] Despite the absence of employment consequences, significant wage increases in the lower half of the distribution including spillovers to individuals not directly affected, are attributed to minimum wages (see for example DiNardo et al. (1996); Lee (1999); Teulings (2003); Autor et al. (2016); Engbom and Moser (2017)). The most promising candidate explanations of the combined wage and employment consequences stem from the search and matching literature. As argued, minimum wages have potential to impact on the returns to extensive and intensive margin search decisions for both unemployed and employed job seekers, impacting equilibrium unemployment and wage distributions.[2]

Given data constraints, much of the existing literature on minimum wages and search has been required to treat search frictions as an unobservable black box. Estimation has relied upon imposing structure on outcomes such as wages

[1]See for example Card and Krueger (2000, 2015), Dube et al. (2010), Kuehn (2016)) for the USA, Stewart (2002, 2004b,a) Dolton et al. (2015) and Dickens et al. (2015), Manning (2016) for the UK, Engbom and Moser (2017) for Brazil, Hyslop and Stillman (2007) for New Zealand and others. It should be noted that some authors have measured negative employment consequences of minimum wages in the USA, for example Neumark and Wascher (2000); Neumark et al. (2007). Such results are normally generated through state-panel methodologies over many years. Other authors, such as Dube et al. (2010), argue that the negative results are a consequence of divergent residual state-level employment trends. UK research almost uniformly finds no large, significant employment consequences.

[2]Seminal work on the topic is Pissarides (1990, 2000).

and employment status to generate hypothesised search responses indirectly, for example Bontemps et al. (1999) and Flinn (2006). Others, such as Dube et al. (2016), prefer less structural methods and instead take stocks and flows between labour market states as indirectly indicative of search behaviours. Outcome data restricts how easily one can separately identify different search mechanism responses. Potential search margin responses could include effort and participation responses of both non-employed individuals versus those wishing to transition between jobs.

This paper provides what appears to be the first direct evidence of minimum wage impacts on multiple search adjustment mechanisms of individuals.[3] I use large survey data on both the extensive margin (searching or not searching) and intensive margin (search effort exerted) for unemployed and on-the-job job seekers. These data are combined with the unique and time-varying age tier structure of United Kingdom minimum wage policy.

The bulk of this paper's analysis focuses on a 2010 change in the age of eligibility for the adult minimum wage from 22 years to 21 years: overnight, the minimum wage for 21 year olds received a boost of nearly 23%. Minimum wages for other age groups changed only in line with inflation, providing a well-identified setting for quasi-experimental analysis. A difference-in-differences identification strategy is used to estimate the treatment effect of the 23% increase in minimum wages for 21 year olds, using non-targeted age groups as controls. I also investigate the introduction of a fourth age category for those aged 25 years and over in early 2016, and the full set of age-specific minimum wage changes since their introduction in 1999.

Headline results find a large increase in minimum wages has no significant impact on employment probabilities but significantly increases the incidence of extensive margin unemployed search. Put differently, a larger proportion of non-working individuals partake in unemployed job search relative to inactivity, boosting labour force participation. Applied to a search framework, an increase in the stock of job seekers reduces firm hiring costs. The zero aggregate employment impacts suggests this is sufficient to outweigh any direct effect of higher minimum wages on firm labour demand. The search-employment results also provide indirect evidence that worker bargaining power and market clearing wages are set too low relative to the levels suggested by the Hosios condition in search theory.[4]

[3] A contemporaneous working paper Adams et al. (2018) investigates search responses to US minimum wages, but is restricted to unemployed search given data constraints.

[4] The Hosios condition states that for market clearing to provide optimal allocations, the relative contribution of worker search and firm vacancy posting to the matching function should

Higher minimum wages might therefore be a second-best approach to address the allocation distortion.

I also find some evidence that average search intensity declines for unemployed job seekers following the minimum wage rise and unemployment durations rise. No statistically significant impacts are found for any search measure associated with on-the-job searching. Potential claims that minimum wages disincentivise progression up a job ladder are not supported by these results.

The contributions of the paper are threefold. Firstly, and most significantly, I provide a direct test of search theory's application to minimum wages and the hypothesised adjustment mechanisms. Secondly, the analysis uncovers stylised facts on search and labour force participation decisions that can be used to guide future search modelling of minimum wage impacts. Finally, the results allow an assessment of the consequences of the highly publicised, recent and planned, increases minimum wages in the UK, from both an unemployment and job ladder perspective.

The rest of the paper is structured as follows. Section 1.2 presents the search framework and testable implications for the empirical analysis. Section 1.3 outlines the empirical context including the UK policy environment, the data and the identification strategies used. Section 1.4 presents the estimated impacts of higher minimum wages on unemployed search, including the extensive and intensive margins of adjustment. Section 1.5 presents the analogous estimates for on-the-job search. Section 1.6 outlines robustness checks undertaken and section 1.7 concludes.

1.2 Framework

1.2.1 Extensive margin search decision - participation

In a baseline search model, such as the one outlined in Appendix A.1, workers are in one of two possible states: employed or unemployed but searching for a job. In reality, many individuals are removed from the labour market (defined as not employed plus not seeking employment and/or not able to work) for a variety of reasons, including caring for family members, studying, and pursuing other non-market activities. Binding minimum wages can only generate zero or positive employment consequences if the baseline search model is augmented to include an extensive margin search decision, elsewhere referred to as a labour supply decision.

equal their relative shares in the matching surplus. If workers' bargaining shares are below this level, equilibrium wages will be lower than socially optimal.

To model the phenomenon, each individual has a flow value of remaining outside the labour market and not searching, ρV_O, where ρ is the discount rate, and V_O the stock value of the outside option.[5] The ρV_O are heterogeneous and follow some distribution Q. An individual decides to enter the labour market if the corresponding value of unemployed search, ρV_U, is higher than their outside option i.e. $\rho V_U \geq \rho V_O$. The proportion of the population engaged in the labour force - the participation rate - is therefore $Q(\rho V_U)$.

Minimum wages enforce a lower bound of m on the feasible wage distribution, and job matches with values less than m do not result in employment. Workers' participation value of search is influenced by the distribution of possible jobs, hence presence of the minimum wage, and workers will now search only if $\rho V_U(m) \geq \rho V_O$.[6] The search participation rate under minimum wages becomes $Q(\rho V_U(m))$ and increases only if the value of unemployed search is increasing in the minimum wage.

The search participation rate directly increases the number of vacancy-worker matches, $M(U, V)$, as M is a positive function of the stock of unemployed searchers U and vacancies V. The rate that a worker is matched with a job is the number of matches over the stock of unemployed, $\lambda_U = M(U, V)/U$. This rate is decreasing in the number of searchers - i.e. $\frac{\partial \lambda_U}{\partial U} < 0$ - which is a congestion externality imposed by job search competitors. The expected duration of job search is the inverse of the job finding rate, which is therefore increasing in the stock of unemployed workers.

From a firm's point of view, the probability that a vacancy will be filled is the number of matches over the vacancy stock, $\lambda_V = M(U, V)/V$, and the expected duration of a vacancy is $1/\lambda_V$. λ_V is increasing in the search participation rate, i.e. $\frac{\partial \lambda_V}{\partial U} > 0$, in effect decreasing the costly probability that a vacancy remains unfilled. It is entirely possible for the value of reduced hiring frictions to outweigh the direct impact of costlier wages on vacancy creation, generating zero or positive employment consequences. Such an employment result is, however, only possible if search participation is increasing in minimum wages.

Implication: *Higher minimum wages can have a non-negative impact on employment only if there is a corresponding increase in unemployed search.*

[5]The form follows Pissarides (2000) and Flinn (2006). The outside option can be conceptualised as a value of leisure, pursuing education, caring for family members and so on.

[6]Minimum wages have several possible effects on the value of unemployed search. Directly, minimum wages lead to the destruction of the lowest productivity jobs (productivity $\theta < m$) and increase wages for those still profitable at m but previously paying less than m. Indirect effects include adjustments to the number of vacancies created and the number of unemployed individuals searching.

1.2.2 Intensive margin search decision - effort

Vacancy-worker matches are also a positive function of the intensity with which workers chooses to search. For an individual worker, their probability of a match, λ_i, is increasing in their own search effort but, through congestion, decreasing in the aggregate search effort, i.e. $\frac{\partial \lambda_i}{\partial s_i} > 0$ and $\frac{\partial \lambda_i}{\partial s} < 0$. Additional search effort is costly for individuals, thus they will only exert effort up until the point where the marginal benefit equals the marginal cost of additional search.

There are two possible effects of minimum wages on search effort, as shown more formally in Appendix A.1. Firstly, search effort is increasing in the offered wage. Minimum wages that increase the wage offer distribution provide a stronger incentive to find employment, increasing the return to search effort. Secondly, however, search effort is decreasing in the ratio of vacancies to unemployed searchers. If minimum wages increase the number of searching individuals competing for jobs, the congestion externality strengthens and the search effort of existing searchers will decrease.

The combined effect influences the overall employment consequences of higher minimum wages. A positive equilibrium effect of minimum wages on aggregate search effort can augment extensive margin search impacts. Contrastingly, a negative effect is likely to be a mitigating force.

Implication: *Higher minimum wages have an ex ante ambiguous impact on search intensity. The direction of change indicates whether the (positive) wage consequences of minimum wages or the (negative) congestion externality dominates.*

1.2.3 On-the-job search

Minimum wages can theoretically impact on on-the-job search and therefore job-to-job transitions up a job ladder. A primary question is whether individuals in low productivity matches invest in searching for a higher productivity match. Workers will choose to search on-the-job if the benefits of doing so outweigh the costs. The benefit is the expected gain from a new job multiplied by the probability it occurs, taking into account that only higher productivity jobs are accepted. The cost is primarily the direct search cost, denoted σ.[7]

The first order implication of binding minimum wages is the reduction in the

[7] In Pissarides (2000), there is also a wage cost from searching. Wages are higher for non-searchers than searchers because searching imposes the cost of a potential quit on the firm. Firms observe whether their workers are searching so can adjust the wage to recoup some of this cost.

value of switching jobs arising from compression in the wage distribution. For jobs with low values of productivity θ, where the minimum wage binds, the current employment value is increasing in the minimum wage and the expected gain from a new job is decreasing. As a consequence we are likely to see fewer job-to-job transitions in particular from low-productivity jobs. Phrased differently, there is concern that minimum wages disrupt the start of the job-ladder model.

Implication: *Higher minimum wages can reduce the incentive to progress up the job ladder, thereby reducing on-the-job search and job-to-job transitions.*

1.3 Empirical setting

Following the abolition of the Wage Councils in 1993, no minimum wage legislation existed in the United Kingdom until the introduction of the National Minimum Wage (NMW) in April 1999. A youth rate, applicable to those aged 18-21 was set at 83% of the adult rate and a lower rate for 16-17 year olds was introduced in October 2003. The stratified levels of minimum wages have been updated annually by a small amount, slightly altering the gap between the adult and youth rate overtime as shown in Figure 1.1.[8] The age categories themselves are adjusted occasionally: on the 1st October 2010 the age of eligibility for the adult minimum wage switched from 22 to 21 years old and on 1st April 2016 a fourth age category was added with those aged 25 and over.

The primary data source used is the Quarterly Labour Force Survey (QLFS). The QLFS is a large survey of households in the UK with detailed demographic, geographic and labour force information on approximately 100,000 individuals each quarter. The labour force data contain information on workers' labour market situation e.g. employment status and history, wages, occupation. Crucially, the QLFS also includes comprehensive and highly detailed data on search behaviour including search methods, intensity and durations. The data are provided for both unemployed and employed job seekers, allowing analysis of unemployed labour supply responses and desired job-to-job transitions. For comparison, the equivalent data for the US data - the Current Population Survey (CPS) - only records search information for unemployed individuals.

A limitation with the QLFS is the accuracy of the wage data arising from individual self-reporting. Wage analysis is therefore carried out using the Annual Survey of Hours and Earnings (ASHE). The ASHE is a 1% sample of employees

[8]A separate minimum wage applies to individuals on apprenticeship schemes however this is not shown in the figure.

Figure 1.1: United Kingdom minimum wage policy structure

	Age 25+	Age 21-24	Age 18-20	Age 16-17	% difference between youth and adult
1 April 2016	£7.20	£6.70	£5.30	£3.87	
	Age 21+		Age 18-20	Age 16-17	
1 October 2015	£6.70		£5.30	£3.87	26.4%
1 October 2014	£6.50		£5.13	£3.79	26.7%
1 October 2013	£6.31		£5.03	£3.72	25.4%
1 October 2012	£6.19		£4.98	£3.68	24.3%
1 October 2011	£6.08		£4.98	£3.68	22.1%
1 October 2010	£5.93		£4.92	£3.64	20.5%
	Age 22+		Age 18-21	Age 16-17	
1 October 2009	£5.80		£4.83	£3.57	20.1%
1 October 2008	£5.73		£4.70	£3.53	21.9%
1 October 2007	£5.52		£4.60	£3.53	20.0%
1 October 2006	£5.35		£4.45	£3.40	20.2%
1 October 2005	£5.05		£4.25	£3.00	18.8%
1 October 2004	£4.85		£4.10	£3.00	18.3%
1 October 2003	£4.50		£3.80	£3.00	18.4%
1 October 2002	£4.20		£3.50	-	20.0%
1 October 2001	£4.10		£3.50	-	17.1%
1 October 2000	£3.70		£3.20	-	15.6%
1 April 1999	£3.60		£3.00	-	20.0%

Source: Low Pay Commission

totaling around 150-200,000 individuals per year. The data on hours and earnings are employer reported from payroll records and response is compulsory. As a consequence it is deemed to have less measurement error than the QLFS.

1.3.1 Regression framework

The key empirical question is the impact of minimum wage levels and subsequent adjustment of the wage offer distribution on the three search mechanisms discussed in section 3. To identify the 'treatment effect' of minimum wages, I use quasi-experimental methodology around age-tier policy changes.

The bulk of the analysis focuses on the change in the eligibility age for the adult minimum wage from 22 years to 21 years on the 1st October 2010. The minimum wage applicable to 21 year olds jumped nearly 23% from the reduced youth minimum wage rate of £4.83 to the adult rate of £5.93 overnight. A difference-in-differences approach is applied, where the 'treatment' group is 21 year olds and the primary 'control' group is 22-23 year olds.[9]

[9]Some attention is paid to the recent introduction of a fourth age tier to those aged 25 and over on 1 April 2016. The minimum wage increased from £6.70 to £7.20 (an increase of around 7.5%) - smaller in financial terms but applicable to a much larger group. Here, those aged 25 and over are the 'treatment' group and those aged 21-24 the most obvious 'control' group.

The baseline regression framework follows the familiar difference-in-differences functional form:

$$Y_{igt} = \alpha_0 + \alpha_1 G_g + \alpha_2 d_t + \delta(G_g * d_t) + X'_{igt}\beta + \epsilon_{igt} \tag{1.1}$$

Y_{igt} is the search outcome of individual i in age-group g at time t. There are two age groups for each policy change: $g \in \{treatment, control\}$. G_g equals one if individual i is in the treatment group ($g =$ treatment) and zero otherwise, d_t equals one if time t is after the policy change and zero if before, and $G_g * d_t$ is an interaction between the two. The difference-in-differences estimate of the treatment effect is the coefficient on the interaction term, δ. Other covariates, X_{igt} can be added in as controls to improve the precision of estimation. If the difference-in-differences strategy is correctly specified controls should not alter the point estimates significantly.

Section 1.6 addresses in detail potential identification concerns surrounding the framework. In short, the approach passes the general tests (parallel trends, no contemporaneous policies etc) and several setting specific concerns.

At times, the analysis is extended to include all minimum wage variation since the introduction. The following functional form captures age threshold changes and the variation in the ratio of adult-to-youth minimum wages:

$$Y_{igt} = \alpha + \lambda G_g + \gamma d_t + \delta log(minwage_{igt}) + X'_{igt}\beta + \epsilon_{igt} \tag{1.2}$$

The only difference to previous regressions is the replacement of the difference-in-differences interaction dummy with the log of individual i's legal minimum wage.

1.3.2 Descriptive statistics

Table 1.1 and Figure 1.2 present descriptive statistics to give context to the core analysis. Table 1.1 shows fractions of the sample engaged in various labour market activities. The sample of individuals are those within 24 months of the policy change in October 2010, calculated for both the regression sample (21-23 year olds) and for the general working age population.

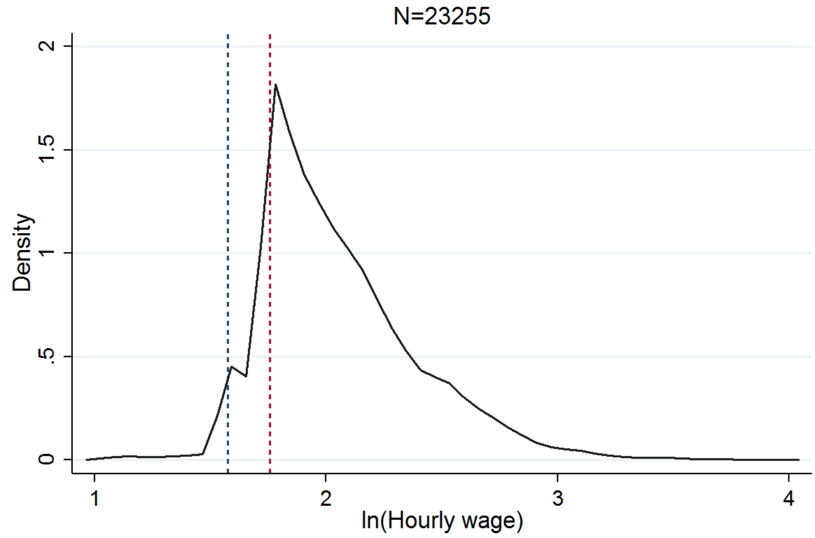
The labour market activities are generated from the QLFS. Two alternative pairs of 'searching'/'not searching' categories are defined as follows. 'Unemployed' refers to the International Labour Organisation definition of unemployment - individuals actively seeking work and available to begin work. 'Inactive' is the complement - individuals who are not actively seeking work and/or unavailable to

Table 1.1: Descriptive statistics

	21-23 year olds		16-64 year olds	
	%	N	%	N
Working	62.68	33,459	69.96	753,728
Unemployed	11.91	6,358	5.73	61,732
Student	14.32	7,645	5.47	58,918
Inactive	11.09	5,919	18.84	203,030
Total	100	53381	100	1,077,408
Not working: Searching	13.29	7,094	6.40	68,961
Not working: Not searching	10.44	5,574	18.45	198,829
Of those working:				
No on-the-job search	86.39	28,953	93.52	705,340
On-the-job search	13.61	4,561	6.48	48,848
Of those searching on the job:				
Want a replacement job	87.69	3,966	83.74	40,468
Want an additional job	12.31	557	16.26	7,855

Table presents sample percentages and counts of individuals within 24months of Oct 2010. Working, unemployed, student and inactive are mutually exclusive and exhaustive categories. Searching and Not Searching have slight definitional changes from Unemployed and Inactive. Source: Quarterly labour force survey: Secure Access.

Figure 1.2: Log(wage) distribution for 18-25 year olds



Kernel density plot of log hourly wage, excluding overtime. The blue and red vertical bars represent youth and adult minimum wage rates respectively. Source: ASHE 2010

begin work. As a consequence, individuals searching for work but unavailable to work are classified as 'inactive'. This subset of non-employed searching individuals is still of interest, so the second 'Searching' category adds these individuals to the ILO 'unemployed' workers to create a full set of non-employed searchers. 'Not searching' is its complement - the remainder of inactive individuals. 'Student' refers to individuals who are inactive as a consequence pursuing educational activities.

Figure 1.2 presents a kernel density graph of the log hourly wage distribution with vertical lines for 2010 youth and adult minimum wages superimposed, calculated prior to the October adult rate change of that year.[10] The two density peaks around the minimum wage rates clearly show the impact of multiple age tiers on the distribution.

Appendix A.3 includes additional descriptives for interested readers. Table A.1 discretises the wage distribution into minimum wage categories, which demonstrates that over 10% - i.e. a sizeable fraction - of 21 year olds earn below the adult minimum wage immediately prior to the policy change (when they, by law, must be paid the higher rate). The directly treated group of 21 year olds is therefore sizeable. Table A.2 presents descriptives on search intensity variables.

[10] These are employer reported weekly wages divided by employer reported paid hours, excluding overtime for both hours and pay, all sourced from the (unweighted) Annual Survey of Hours and Earnings, 2010.

1.4 Unemployed search results

As discussed in the framework section, for minimum wages to have no employment consequences there must be an increase in unemployed search. Therefore, the core analysis begins with estimating the impact of the increased minimum wages on the probability of employment and unemployed search behaviour.

The baseline extensive margin search results asks whether non-working 21 year olds switch from not-searching (inactivity) to searching (unemployment) in response to a 23% increase in the minimum wage in 2010, and whether employment rates are affected. A system of linear probability models, using the difference-in-differences identification, is estimated on the mutually exclusive and exhaustive set of labour market outcomes - working, unemployed (i.e. searching) and inactive (i.e. not searching).[11] Errors are clustered at the age-region level to account for non-spherical errors associated with difference-in-differences.[12] I include all individual observations 24 months either side of the policy change (1st October 2010) who are aged between 21 and 23 years of age.[13]

Table 1.2 presents the baseline unemployed search results for the linear probability system. I control for individuals' sex, region of residence (defined at the NUTS2 level), the quarter of the observation, various measures of educational attainment, marital status, ethnicity and occupation.[14] I also include a variable referred to as 'proxy' which controls for whether the survey was a proxy response by a family member rather than the individual themselves, known to introduce more measurement error.

The estimated treatment effect for the probability of employment is insignificant and small, implying the higher minimum wage has no measurable impact on the propensity for 21 year olds to be in work. A zero employment consequence result is in line with most of the previous UK minimum wage literature.

Consistent with search theoretic explanations of a zero employment consequence, we see a corresponding significant increase in unemployed search and a decrease in search inactivity. The result should be interpreted as higher mini-

[11]Probit and multivariate logit models are also estimated and produce similar estimates. Given the ease of interpretation, and weaker identification requirements, the paper presents the linear probability versions. Athey and Imbens (2006) discuss the additional error structure assumptions for identification in non-linear difference-in-differences models.

[12]Bertrand et al. (2004). Age is defined in years, region is defined by the NUTS2 level geographic region of residence. This gives 117 clusters.

[13]For most of the analysis, individuals removed from the labour market due to full time education are omitted. Robustness checks verify that this is not driving the results.

[14]Table A.3 in Appendix A.4 presents the results without any controls - reassuringly the inclusion of controls does not change the treatment estimates.

Table 1.2: Baseline unemployed search estimates - extensive margin

	(1) Working	(2) Unemployed	(3) Inactive
Post	-0.0222***	0.0110**	0.0112**
	(0.00588)	(0.00429)	(0.00438)
Age 21	-0.0327***	0.0269***	0.00588
	(0.00788)	(0.00602)	(0.00527)
Post*Age 21	-0.00403	0.0205**	-0.0164**
	(0.0110)	(0.00835)	(0.00744)
Constant	0.607***	0.207***	0.185***
	(0.0296)	(0.0151)	(0.0216)
Observations	45736	45736	45736
Controls	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

mum wages increasing the share of extensive margin search among non-working individuals. The increase is in the order of two percentage points from a base of around twelve percentage points (from the descriptive statistics), so is therefore economically significant too.

The analysis is also repeated with the alternative pairing of 'searching'/'not searching' variables - including searching but unavailable workers into the 'searching' category. The results are unchanged. Secondly, I use an entirely separate variable from the Quarterly Labour Force Survey that asks individuals, under a different question, whether they have been searching for work at any point in the last four weeks. Again, the results find an increase in extensive margin search in response to the minimum wage increase. Taken together, the results do not appear to be driven by data definition quirks.

An amount of analysis was undertaken to test if the treatment estimates varied by educational attainment, region of residence and gender. One would expect minimum wage policy to impact less educated groups more strongly than highly educated groups. One might also expect estimated impacts of minimum wages to be higher in low wage areas, where the minimum wage is more locally binding. By stratifying the sample on education and regional income levels, it was found that low education individuals are driving the baseline results for unemployed search.

Put differently low educated workers appear far more impacted by the minimum wage than their highly educated counterparts. The results however did not vary significantly across regions by local median wages. There was some evidence that the extensive margin search response of men was slightly stronger than that of women, but overall both genders exhibit similar patterns.

Concern may be raised about the external validity of focussing on a single, albeit clean, minimum wage wage change. To address the issue, I also re-ran the difference-and-differences analysis on the high profile 2016 introduction of the National Living Wage - effectively a higher minimum wage for those aged 25 and over. I then extended the analysis to include all minimum wage variety since the introduction in 1999 - effectively all the age-tier changes and upgrading - confirming the findings.

1.4.1 Unemployed search intensity

The above results suggest a robust increase in the number of unemployed searchers in response to the higher minimum wage for 21 year olds in 2010. The next question is to ask how this impacted on the search effort exerted by unemployed searchers.

'Effort' by its very nature is a rather intangible concept. Using the Quarterly Labour Force Survey search data, I have constructed what can be thought of as noisy measures of an individual's search effort. These are available for both unemployed job seekers and employed job seekers, searching on-the-job.

In the QLFS, individuals who have already acknowledged that they are seeking a job then list off the primary and secondary methods by which they seek a job. Fourteen different search methods are included in the tabulated results, including 'Visit a Jobcentre', 'Study situations vacant', 'Ask friends, relatives, colleagues' and 'On books at a private employment agency.' Individuals indicate their main search method followed, in decreasing order, by any other search methods they use.

The first summary measure of 'search effort' is a simple count of the number of methods an individual uses to search. The logic is that if an individual indicates that they are searching using multiple methods, they are likely to be investing more effort than if only searching with a single measure.

The majority of respondents (90%) only acknowledge one search method and there is a long tail of respondents acknowledging many search methods. In response, the second summary measure is simply a binary variable equal to one if the individual acknowledges more than one method.

The third measure categories the level of effort based on the main method acknowledged. Many of the options can be deemed 'low commitment', or passive, search methods such as 'On books of private employment agency,' 'Wait for results of application.' Others are more likely to require considerable effort exertion and can therefore be considered active search methods. For example 'Answer job advertisements,' 'Apply directly to employers.' All fourteen answers were categorised as either passive or active, with the full list in Appendix A.2. A binary variable, 'Active', was created that equals one if the main search method used is active, and zero if passive.

I begin by investigating the response of unemployed search intensity to the 2010 minimum wage change. Again, the baseline difference-in-differences method is used and those aged 21-23 years old and surveyed 24 months either side of the policy comprise the core sample.

Table 1.3: Unemployed search intensity estimates - intensive margin

	(1)	(2)	(3)	(4)
Outcome:	Active	>1 methods	# methods	# methods
Estimation:	OLS	OLS	OLS	Poisson
Post	-0.0451***	0.0101	0.0292	0.0192
	(0.0153)	(0.00907)	(0.0476)	(0.0261)
Age 21	0.0298*	0.00911	0.0394	0.0267
	(0.0175)	(0.0104)	(0.0546)	(0.0299)
Post*Age 21	-0.0168	-0.0262*	-0.112	-0.0789*
	(0.0242)	(0.0144)	(0.0754)	(0.0416)
Constant	0.643***	0.234***	2.120***	0.838***
	(0.0592)	(0.0352)	(0.185)	(0.100)
Observations	6990	6990	6990	6990
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are not in work and are searching for a job. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.3 presents the initial results by estimating whether unemployed job-seekers search more intensely in response to the minimum wage change. Columns one and two are the classic linear probability model using the binary variables 'Active' and 'Multiple search methods' respectively. Negative treatment point estimates are estimated, but only the second is weakly significant. Columns three

Table 1.4: Unemployed search intensity estimates - correcting for selection

	(1)	(2)	(3)
Outcome:	Active	>1 methods	# methods
Post	-0.0441***	0.00983	0.0237
	(0.0151)	(0.00896)	(0.0474)
Age 21	0.0299*	0.00750	0.0368
	(0.0173)	(0.0103)	(0.0545)
Post*Age 21	-0.0202	-0.0272*	-0.125*
	(0.0240)	(0.0142)	(0.0753)
Constant	0.668***	0.259***	2.291***
	(0.0597)	(0.0353)	(0.188)
Select eq.			
Post	0.0324	0.0324	0.0344
	(0.0277)	(0.0277)	(0.0278)
Age 21	0.0964***	0.0964***	0.0861***
	(0.0317)	(0.0317)	(0.0318)
Post*Age 21	0.0755*	0.0755*	0.0946**
	(0.0443)	(0.0443)	(0.0444)
Constant	0.115	0.115	0.0737
	(0.102)	(0.102)	(0.103)
Lambda	-0.0575***	-0.0371***	-0.230***
	(0.0169)	(0.0100)	(0.0527)
Observations	19922	19922	19922
Controls	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are not in work. The selection equation includes all controls and treatment variables alongside a variable for studying - the exclusion restriction. Heckman selection model estimated by two-step maximum likelihood. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and four use the actual number of search methods reported, estimating the model with standard OLS and maximum likelihood Poisson respectively. Given the low count data nature of the variable, column four is likely a better specification. Column four again finds negative and weakly significant treatment estimates.

Table 1.3 only includes those already searching for a job and thus is potentially susceptible to selection bias. In response, Table 1.4 present Heckman selection corrected regressions.[15] Consistent with the extensive margin search results, a significant positive treatment effect is estimated in the probit selection equation. Intensive margin results are similar to those with no selection correction: negative point estimates are found all around, but these are only weakly significant for the second and third columns: the variables for using multiple search methods. The significance of the mills lambda estimates suggests that selection is an important part of the regression fit, however excluding it does not appear to cause bias for any outcome variable investigated.

Tables A.7 and A.8 in Appendix A.4 repeat the above regressions on stratifications of the sample by education and regional income. As for the extensive margin results, it appears that the negative treatment estimates are driven by low education individuals. Highly educated individuals do not have any significant treatment estimates. Again, no major differences between higher and lower median wage regions are uncovered.

1.4.2 Unemployed search duration

Job seeking duration is an additional search moment for which to test the theory. Tables 1.5 and 1.6 present the standard difference-in-differences regressions for self-reported unemployed job seeking durations. Durations are provided at the point of the QLFS interview not at the point of finding a job therefore the job seeking is ongoing. Respondents are grouped into discrete time categories such as “less than one month" and “between one and three months" rather than reporting precise durations, thus the mid-point of each time category is taken as the approximated time spent searching for each individual respondent. Two outcome variables are used in separate instances: one is the self-reported unemployment duration (as, by definition, unemployment must involve job-seeking) which is referred to as TimeA. Respondents are separately asked how long they have been searching for a job, and this outcome variable is referred to as TimeB. TimeB is generally shorter

[15] As is common place with Heckman selection models, an exclusion restriction is used: the variable ‘Student’ is included in the selection equation but not the intensive margin equation. It seems realistic that full time studying should impact on an individual’s decision to search or not, but perhaps less so on how hard they search once deciding to search.

than TimeA. Reassuringly, both responses give qualitatively identical results.

Table 1.5 presents pure linear regressions of the expected time spent job-seeking while Table 1.6 formally corrects for those selecting into searching. Significant, positive treatment estimates are found for both the selection equation (implying more individuals search in response to the higher minimum wage, consistent with previous findings) and the intensive margin duration equation. More individuals may be searching, but on average they are searching for longer. When stratified on education level (in Appendix A.4), again the duration results appear to be mostly driven by low education individuals. As for the search intensity results, none of the intensive margin treatment estimates appear biased by the exclusion of a selection correction - all regressions were run with and without selection correction and no significant differences were uncovered for the estimated treatment effect.

Table 1.5: Unemployed search duration estimates

	(1) TimeA	(2) TimeA	(3) TimeB	(4) TimeB
Post	0.895	1.418**	1.265*	1.841**
	(0.703)	(0.704)	(0.714)	(0.724)
Age 21	-1.329*	-1.518**	-1.384*	-1.609**
	(0.727)	(0.616)	(0.738)	(0.632)
Post*Age 21	2.985***	2.529**	2.861***	2.394**
	(1.031)	(0.977)	(1.058)	(1.007)
Constant	[Witheld]	19.14***	[Witheld]	20.86***
		(1.875)		(2.229)
Observations	5096	5096	5087	5087
Controls	No	Yes	No	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. TimeA is unemployment searching duration, TimeB is job-seeking duration, both self-reported. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity, occupation. The constants in columns 1 and 3 are witheld according to the UK Data Service statistical disclosure controls.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6: Unemployed search duration estimates - selection correction

	(1)	(2)	(3)	(4)
Outcome:	TimeA	TimeA	TimeB	TimeB
Post	3.064***	1.362*	1.275**	1.587***
	(0.547)	(0.699)	(0.565)	(0.562)
Age 21	-0.188	-1.539**	-1.340**	-1.261**
	(0.728)	(0.611)	(0.666)	(0.516)
Post*Age 21	2.905***	2.504**	2.335***	1.848**
	(0.921)	(0.974)	(0.867)	(0.839)
Constant	[Witheld]	19.43***	[Witheld]	21.85***
		(1.919)		(1.775)
Selection eq.				
Post	0.169***	0.225***	0.0330	0.0199
	(0.0277)	(0.0329)	(0.0325)	(0.0322)
Age 21	0.00249	0.0664*	0.0667	0.0660*
	(0.0378)	(0.0387)	(0.0439)	(0.0379)
Post*Age 21	0.188***	0.168***	0.102**	0.122**
	(0.0475)	(0.0559)	(0.0476)	(0.0497)
Constant	[Witheld]	0.355***	[Witheld]	0.168*
		(0.0985)		(0.0918)
athrho				
Constant	2.951***	-0.0368	-0.149***	-0.0566***
	(0.185)	(0.0293)	(0.0203)	(0.0207)
lnsigma				
Constant	2.923***	2.588***	2.641***	2.564***
	(0.0248)	(0.0251)	(0.0257)	(0.0250)
Observations	19922	19922	19922	19922
Controls	No	Yes	No	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity and occupation. TimeA refers to self-reported unemployment searching duration. TimeB is a separate self-reported measure of time spent job-seeking. The two are similar, but generally TimeB is shorter than TimeA. The constants in columns 1 and 3 are witheld according to the UK Data Service statistical disclosure controls.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.4.3 Relating the unemployed search results to theory

To summarise and relate back to the search theory implications: the results have found no change in the probability of employment and the required corresponding increases in unemployed searchers. In this situation, it appears that minimum wages increase the value of unemployed searching and the corresponding increase in unemployed searchers prevents employment destruction. Put differently, the positive labour supply response, and the subsequent reduction in firm hiring costs, is sufficient to outweigh any direct negative effect on firms' labour demand. The increased search concurrent with zero employment impacts is also indirect evidence that the Hosios condition is not satisfied.[16] In particular, the results suggest that worker bargaining power (in the low wage portion of the labour market) is set too low relative to workers' search contribution to the matching function. Market clearing wages and employment levels are below the socially optimal allocated. Higher minimum wages can provide a second-best redress in such a situation.[17]

There is some evidence of decreased average intensity, which if robust, suggests that the congestion externality of more searchers dominates the direct effect of higher wages. Overall, the returns to search effort appear to have decreased. Consistent with more searchers (congestion) and potentially lower search effort, the duration of unemployed search increases.

1.5 On-the-job search

As discussed in the framework section, search and matching models of job ladders would suggest that minimum wages may disrupt on-the-job search, and hence the job ladder, by weakening incentives to progress to higher productivity matches.

To test this hypothesis, as before, I use a combination of difference-in-differences identification strategy around policy changes and pooled regressions of the entire set of minimum wage variations. An individual is categorised as searching on the job if they answer affirmatively to whether or not they are looking for an additional paid job or business. If they are, they then clarify whether it is to be an additional job or a replacement job for their current position. The vast majority of respondents are looking for a replacement job - consistent with a job ladder model.

[16] The Hosios condition states that for market clearing to provide optimal allocations, the relative contribution of worker search and firm vacancy posting to the matching function should equal their relative shares in the matching surplus.

[17] The theoretical framework and relevance of the Hosios condition to minimum wages is discussed in more detail in Flinn (2006).

Table 1.7: Baseline on-the-job search estimates - extensive margin

	(1) OJS (all)	(2) OJS (all)	(3) OJS (new)	(4) OJS (new)	(5) Replace
Post	0.0237***	0.0208***	0.0235***	0.0205***	0.0196
	(0.00593)	(0.00582)	(0.00473)	(0.00465)	(0.0139)
Age 21	-0.00615	-0.00456	-0.00963	-0.00765	-0.0239
	(0.00835)	(0.00783)	(0.00720)	(0.00669)	(0.0191)
Post*Age 21	0.00488	0.00576	0.00387	0.00426	-0.00864
	(0.0122)	(0.0123)	(0.0103)	(0.0104)	(0.0240)
Constant	[Witheld]	0.187**	[Witheld]	0.201**	0.993***
		(0.0933)		(0.0927)	(0.0582)
Observations	33392	33392	33354	33354	4488
Controls	No	Yes	No	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are in work. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity and occupation. Columns 1-2 are LPM with the dependent variable equal to one if an individual is searching for any job. Columns 3-4 are LPMs for an individual searching for a replacement job. Column 5 is, of those searching for a job, the likelihood of searching for a replacement job not an additional job.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: On-the-job search intensity estimates - intensive margin

	(1) Active OLS	(2) >1 methods OLS	(3) # methods OLS	(4) # methods Poisson
Post	-0.0392**	-0.00326	-0.0647*	-0.0534
	(0.0161)	(0.00898)	(0.0372)	(0.0335)
Age 21	0.0317	0.00675	-0.00420	-0.00388
	(0.0210)	(0.0118)	(0.0487)	(0.0434)
Post*Age 21	-0.0161	0.00138	0.0903	0.0736
	(0.0287)	(0.0160)	(0.0664)	(0.0592)
Constant	0.333**	0.230***	1.643***	0.519*
	(0.132)	(0.0736)	(0.305)	(0.272)
Observations	4529	4529	4529	4529
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are in work and are searching for a new job. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7 presents baseline results linear probability models for the estimated treatment effect of the 2010 policy change on the propensity to search on the job. Columns 1 and 2 use a dependent variable Y_{igt} that refers to the default measure of on-the-job search, labelled OJS. This equals one if the individual is undertaking any form of on-the-job search. Column 3 and 4 redefines on-the-job search as only occurring if the individual is looking for a replacement job and zero if the are either not searching, or searching for an additional job. This brings the definition more in line with the notion of a job-ladder. Finally, column 5 investigates whether those individuals already searching are more likely to search for a replacement job versus an additional job following the policy change. There, the dependent variable 'Replace' equals one if the job they are searching for is intended to replace their existing one and zero if it is in addition to their existing job. As can be observed, all estimated treatment effects are not statistically distinguishable from zero.

Similar to the unemployed search analysis, I further investigate whether the estimated treatment effects vary by sub-populations. I interact the treatment term with educational attainment, and split the sample into high and low educated individuals as shown in Table A.11 in Appendix A.5. There is no statistical

Table 1.9: On-the-job search intensity estimates - correcting for selection

	(1) Active	(2) >1 methods	(3) # methods
Post	-0.0256	-0.0186	0.143
	(0.0720)	(0.0405)	(0.202)
Age 21	0.0287	0.0101	-0.0501
	(0.0262)	(0.0151)	(0.0891)
Post*Age 21	-0.0123	-0.00283	0.147
	(0.0347)	(0.0201)	(0.121)
Constant	0.0655	0.531	-2.432
	(1.382)	(0.775)	(3.782)
Selection eq.			
Post	0.103***	0.103***	0.103***
	(0.0220)	(0.0220)	(0.0220)
Age 21	-0.0231	-0.0231	-0.0231
	(0.0277)	(0.0277)	(0.0277)
Post*Age 21	0.0274	0.0274	0.0274
	(0.0387)	(0.0387)	(0.0387)
Constant	-1.088***	-1.088***	-1.088***
	(0.196)	(0.196)	(0.196)
Lambda	0.167	-0.188	2.540
	(0.857)	(0.480)	(2.332)
Observations	33459	33459	33459
Controls	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are in work. The selection equation includes all controls and treatment variables. Heckman selection model estimated by two-step maximum likelihood. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

difference in the estimated treatment effects, either in the split sample or the interaction term regressions. I also test to see whether the estimated treatment effect varies by regional wages. These results are presented in Tables A.12 and A.13 in Appendix A.5. Again, the answer appears to be that all regions have statistical zero estimated treatment effects.

In short, no evidence of causal impact of minimum wages on on-the-job search is uncovered, either overall or in any sub-population. It appears that, at least in this setting, no significant impact on the intent to change jobs can be uncovered.

Table 1.10: On-the-job search duration estimates

	Baseline		By education		By regions	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Low	High	Poor	Rich
Post	1.745***	1.806***	1.892***	1.457***	1.987***	1.371**
	(0.399)	(0.395)	(0.601)	(0.475)	(0.624)	(0.574)
Age 21	-0.764*	-1.292***	-1.044*	-1.942***	-1.540***	-1.112*
	(0.441)	(0.381)	(0.539)	(0.590)	(0.532)	(0.578)
Post*Age 21	-0.721	-0.455	-0.383	-0.444	-0.329	-0.727
	(0.599)	(0.573)	(0.779)	(0.965)	(0.839)	(0.870)
Constant	[Witheld]	18.82***	26.47***	6.267***	28.73***	8.915***
		(5.078)	(7.101)	(1.916)	(8.367)	(2.447)
Observations	4503	4503	2584	1891	2206	2090
Controls	No	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. TimeA is unemployment searching duration, TimeB is job-seeking duration, both self-reported. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity, occupation. Columns include 1-2 all individuals, 3-4 split the sample by educational attainment, 5-6 by regional income.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Despite not finding evidence that minimum wage changes impact on employed individuals' propensity to search on the job, there remains a possibility that search effort changes for those already searching. Tables 1.8 and 1.9 repeat the search intensity analysis for those individuals employed and potentially searching on-the-job. The same search measures are used and, again, a mixture of regressions that include only those already searching (ignoring selection) and those that deal with selection are presented. The sample is stratified along education and regional income lines. Nowhere do I find a significant treatment estimate of minimum wages for on-the-job search intensity. It appears that any adjustment is again restricted to unemployed job seekers.

Table 1.11: On-the-job search duration estimates - selection correction

	Baseline		By education		By regions	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Low	High	Poor	Rich
Post	2.111***	-1.613	-9.262	2.681	-1.827	-1.223
	(0.332)	(3.375)	(37.17)	(2.060)	(5.016)	(4.331)
Age 21	-0.866*	-0.522	-3.555	-3.502	-2.431	3.047
	(0.457)	(1.577)	(9.820)	(2.676)	(2.103)	(6.717)
Post*Age 21	-0.641	-1.378	-2.366	-0.371	-0.527	-4.403
	(0.631)	(2.128)	(9.868)	(1.283)	(2.435)	(6.501)
Constant	[Witheld]	89.66	212.8	-12.80	88.65	78.54
		(66.39)	(616.1)	(31.10)	(76.17)	(105.2)
Selection eq.						
Post	0.111***	0.0946***	0.0955***	0.108***	0.122***	0.0600*
	(0.0206)	(0.0216)	(0.0281)	(0.0346)	(0.0312)	(0.0313)
Age 21	-0.0308	-0.0216	0.0212	-0.133**	0.0277	-0.0968**
	(0.0267)	(0.0278)	(0.0327)	(0.0554)	(0.0402)	(0.0406)
Post*Age 21	0.0238	0.0249	0.0181	0.00181	0.00492	0.0859
	(0.0377)	(0.0388)	(0.0461)	(0.0782)	(0.0556)	(0.0571)
Constant	[Witheld]	-1.030***	-0.718***	-0.618*	-0.981***	-0.690***
		(0.194)	(0.262)	(0.357)	(0.285)	(0.256)
Lambda	4.067***	-45.41	-142.1	15.13	-39.59	-53.72
	(0.150)	(42.11)	(469.0)	(24.26)	(49.66)	(80.42)
Observations	33459	33459	23593	9482	15814	15677
Controls	No	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are in work. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity and occupation. TimeA refers to self-reported unemployment searching duration. Model is a Heckman selection model estimated by two-step maximum likelihood.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, Tables 1.10 and 1.11 repeat the durations analysis for on-the-job search durations. Only one outcome variable is available; the self-reported duration of search, equivalent to TimeB in the unemployed job-seeking analysis. No statistically significant effects are found either on the intensive or extensive margin. Again this is fully consistent with earlier on-the-job search results.

1.5.1 Relating the on-the-job search results to theory

Interestingly, there is no on-the-job search measure - probability of searching, search effort or duration of searching - that produces significant treatment estimates from minimum wages. A statistically zero result such as this is important in its own right. It should be interpreted as there being no measurable impact of minimum wages on a worker's desire to transition jobs. Here, at least, it appears that concerns over higher minimum wages incentivising individuals to remain in low productivity jobs are not substantiated.

1.6 Robustness checks

As for any difference-in-differences identification strategy, the underlying assumptions are tested where possible. I test the common trends identification requirement by looking for statistically significant differences in time trends between treatment and control groups prior to the policy change. I do this in a number of ways. Firstly, outcomes of interest are regressed on an intercept, the treatment dummy, a collection of time dummies d_t and those time dummies interacted with the treatment group, $d_t * G_g$. Significant coefficients on the interaction terms, $\alpha_{2,\tau}$, leading up to the policy change would indicate that the treatment and control groups were diverging prior, a likely violation of the common trends assumption.

To formalise, for each outcome variable of interest, the following regression was run for 21-23 year olds, our sample of interest:

$$Y_{igt} = \alpha_0 + \alpha_1 Age21 + \sum_{\tau=-T}^{T} \beta_\tau d_\tau + \sum_{\tau=-T}^{T} \gamma_\tau (Age21 * d_\tau) + \epsilon_{igt} \tag{1.3}$$

These were undertaken for both annual time dummies and quarterly time dummies. When it came to quarterly regressions, seasonal fixed effects had to be taken into account. [18] Once quarterly fixed effects were included, no significant interaction terms were uncovered in the four years leading up to the policy change

[18] More 21 year olds are in full time education than 22-23 year olds and thus significant differences in time dummies for each summer quarter (during summer break) were uncovered.

when all the primary outcome variables (detailed below) were tested. From five years prior, there was some measured minimal divergence which is not overly surprising given the time lag.

Analysis also checked for diverging parametric time trends by fitting separate linear and quadratic time trends for the treatment and control group. Once controlling for group fixed effects, again no statistically significant differences were uncovered in the four years leading up to the policy change.

As a further robustness check, placebo difference-in-differences regressions were run on data at alternative time periods. False interventions were generated for various time periods within a four year range either side of the true policy intervention (ensuring that the true policy intervention was not captured). None of the false interventions generated significant treatment estimates.

I was also able to test for observable composition changes in the treatment and control groups that might conflate demographic change with the policy intervention. Difference-in-differences regressions were run were with the outcome being various observable group characteristics e.g. ethnicity, gender, geographic location, educational attainment. None were found to have significant, diverging results between the treatment and control groups, which is an encouraging result. By definition, there is no way of testing changes in unobservable characteristics that may influence labour force outcomes. As I am comparing 21-23 year olds - a very narrow demographic band in the population - it seems reasonable to assume that a major unobservable change differentially affecting one group is unlikely.

There are a couple of other considerations for identification. One must be sure that the control group of 22-23 year olds is indeed a control group - they cannot be impacted by the treatment. I considered this in detail by using difference-in-differences methodology with 22-23 year olds as the treatment group, and various sets of other age groups as the relative controls. Under no specifications were significant treatment estimates on 22-23 year olds measured.

Nonetheless I also varied the age of the control group used, out of concern that 22-23 year olds might still be impacted. The results were robust to using any control group of twenty-something year olds.

In this particular setting, the treatment and impact on the results of students may be of concern. Many 21 year olds are still in formal education and the baseline results exclude labour force inactive students from the sample. To assuage concerns that this decision impacts on the results, all regressions were run including students into the analysis. For no outcome did the results change quantitatively meaningfully. Combined with the earlier findings that the results are driven by less educated individuals - i.e. those with no post-school education - this should reassure those with concerns.

Finally, there is always concern that the estimated results should instead be attributed to a concurrent policy change. I was unable to find any relevant policy change around the 2010 mark that affected 21 year olds differentially to 22-23 year olds. The majority of other age discontinuities in labour market policies kick in at 18 or 25 years old. No other policy impacts were found differentially affecting 21 year olds compared to 22-23 year olds.

As a matter of functional form robustness, all linear probability models were also run as non-linear probit and/or multivariate logit models. This did not qualitatively change the results but, naturally, decreased the ease of estimate interpretation.

1.7 Conclusion

Taken together, the analysis finds robust responses to minimum wages for unemployed searchers. There is a shift from inactivity (no search) towards labour force participation, specifically unemployed searching, in response to the 23% boost in minimum wages of 21 year olds in 2010. The increase in search, a form of labour supply response, appears sufficient to outweigh any direct effect of higher minimum wages on firm labour demand. The net effect of the two generates no significant impact on employment rates. However, due to increased search congestion, the increased extensive margin search is accompanied by a corresponding increase in the average duration of unemployed search.

Surprisingly, I also find weak evidence of a decrease in average search effort for unemployed searchers. This has three potential explanations. Firstly, decreased average search effort could be due to a composition effect: marginal searchers switching into searching at a low intensity drag the average down. Alternatively, the increase in extensive search generates a congestion externality on existing searchers which in turn may decrease in their search effort. Thirdly, search intensity may play a valuable role in assisting workers to find their optimal job match. Minimum wages that increase wages for the lowest paying jobs may decrease the returns to an optimal match from the worker's point of view, discouraging costly search effort. In short, minimum wages may create an 'any job will do' mentality, reducing match qualities. Each of these three explanations has significant ramifications for labour markets, and merit further consideration.

In contrast to the unemployed search margins, no significant impacts on any measures of on-the-job search are found. No change in the propensity to search, effort of searching or duration of searching is estimated. It appears that, at least in this setting, minimum wage increases do not impact on worker's intentions to progress up the job ladder, assuaging a possible concern that minimum wage policies incentivise individuals to remain in low productivity jobs.

Chapter 2

Localised employment spillovers

Abstract

This paper is the first to provide firm level estimates of the propagation rates of localised employment shocks through space and time. A spatial network of the universe of UK firms with near pinpoint location accuracy is used to estimate the firm-level employment adjustment to mass layoffs. Results show that firm level employment adjustment is highly localised and decays rapidly through space - the negative spillover effects halve approximately every kilometre further away from the event. Firm level adjustment is also highly persistent, with further localised employment losses continuing for at least five years after the event. The spillover effects are experienced by a wide range of local firms, but are strongest in non-tradeable sector firms, consistent with the presence of local product demand transmission mechanisms. The paper provides new supporting evidence to theories that sluggish firm level adjustment interacting with local agglomeration forces generate persistence in local labour market outcomes. Furthermore, the micro-level effects uncovered are extremely localised, and thus more standard analysis methods discretising space into regions will incur significant measurement costs.

Keywords: local employment dynamics, spillover decay rates, agglomeration
JEL codes: J23, J63, R12

2.1 Introduction

There is substantial path dependence in local economic fortunes: economic inequalities between regions are highly persistent and, in some places, diverging over time.[1] These dynamics provide somewhat of a puzzle, as the traditional spatial equilibrium framework would suggest that localised shocks dissipate through factor adjustment and regions converge overtime. Focussing on the labour market, recent explanations have suggested that an initial labour demand shock is strengthened into a larger, more permanent shock (Amior and Manning, 2018). The conversion to a permanent shock may occur at the local firm level as sluggish firm adjustment interacts with local agglomeration forces (Dix-Carneiro and Kovak, 2017). To date, little is known about the strength through time and space of employment shock propagation at the firm level because the existing literature has approached the question of local employment dynamics by aggregating across firms, geography or both.

This paper provides the first firm level estimates of the propagation rates through space and time of localised employment shocks. Doing so demonstrates that an initial shock is indeed converted into a persistent shock as individual firms located in close proximity to an initial adverse employment shock continually reduce employment for many years after the event. The paper addresses 1) how localised are employment spillovers and how rapidly do these decay through space, as well as 2) what are the dynamics at the firm level of the shock propagation – do they exhibit continual employment adjustment or does the firm level shock dissipate overtime? The relevance and spatial scale of potential channels, including input-output links, local product demand, labour market spillovers, and within-industry knowledge spillovers are also considered.

The paper is able to answer such questions by approaching the problem in a way novel to the established literature. The predominant approach when analysing spatial variation in any economic outcome is to discretise space into mutually exclusive and exhaustive units.[2] This may be due to data restrictions – observations are generally allocated an administrative unit rather than a precise location – or because spatial aggregation simplifies analysis. However, when the object of interest is spatial spillovers, any form of discretisation will not be innocuous. There is an inherent tradeoff in the scale of discretisation. If the unit

[1] Moretti (2011) provides a summary of the literature.

[2] See for example Dix-Carneiro and Kovak (2017); Amior and Manning (2018); Dube et al. (2010); Autor et al. (2013); Acemoglu et al. (2016), and many others, for a selection of applications.

size is too small, spillovers will extend into neighbouring units, contaminating control units. If set too large, the spillovers operate only in a small fraction of the unit and the estimates will likely average to near zero. The estimated result is therefore fundamentally dependent on the discretisation. Unfortunately, without knowledge of the underlying scale of the effects it is impossible to know where on the continuum one's analysis lies.

In contrast, this paper bypasses the inherent cost presented in standard methods by treating space as continuous. A spatial network of all firms in the UK is constructed using near pinpoint location accuracy from the Business Structure Database (BSD). The response of the firm level network to localised adverse employment shocks is then assessed. Mass layoffs, defined as more than 1,000 workers lost in a given year (with some caveats), are used as a localised employment shock event. Each non-masslayoff firm is linked to their closest masslayoff events in each year. The primary feature of interest is the relationship between geographic proximity to a masslayoff, measured in Euclidean distance, and subsequent firm level employment behaviour.

Employment spillovers from masslayoff events are strong and very highly localised. A firm located very close to a masslayoff loses, on average, approximately 7% of their employment. This employment loss exhibits strong spatial decay by abating rapidly with distance. The effect approximately halves for every kilometer further away from the events, becoming very small for firms located further than about 5 kilometers from a masslayoff.

The dynamic analysis finds that further firm level employment losses continue for at least five years after the masslayoff event. These subsequent effects also exhibit the strong spatial decay pattern. The annual effects compound over time such that the longer term firm level impact is much larger than the initial response to the shock. This provides firm-level microfounding evidence to support observations that the longer term effects exceed immediate effects at the regional level (Dix-Carneiro and Kovak, 2017).

A number of possible spillover channels to explain these effects are considered. In particular, I address several potential sources of agglomeration spillovers put forward by the literature. Firstly, I consider whether firms in similar industry, or similar labour markets, to nearby mass layoffs are more strongly affected by spillovers.[3] These two features are often through to indicate knowledge sharing, which in turn can generate productivity (and employment) spillovers following

[3]These features can also be referred to as 'industrial closeness' and 'labour market closeness' respectively.

shocks. I also consider whether industries with input-output linkages to mass layoff firms might experience stronger spillovers. Lastly, I investigate the relevance of local product demand spillovers by testing for differences in responses between tradeable and non-tradeable firms.

Neither the spillovers nor their strong degree of localisation appear confined to a particular subset of firms or firm-masslayoff pairs. A wide range of firm types experience strong, localised enploymnent spillovers. However, firms in non-tradeable sectors experience stronger spillovers than the tradeable counterparts. The stronger non-tradeable response points to the relevance of local product demand spillovers following initial employment shocks.

The contributions of the paper are fourfold. Firstly, I provide the first direct estimates on the degree of localisation of negative employment spillovers - employment spillovers from adverse employment events, here taken to be masslayoffs, are highly localised. This pattern has not been directly uncovered to date as it requires the use of precise location data. The degree of localisation is stronger than had been anticipated, either explicitly or implicitly in the employment spillovers literature. The results show that firm level employment spillovers are much like many other economic activities considered by other literatures; the micro effects are highly localised.[4]

Secondly, the paper sheds additional light on the dynamics behind localised employment adjustment. The firm level dynamic results provide the first empirical support to the theory that labour shocks are transmitted at the firm level, possibly through delayed firm adjustment interacting with agglomeration forces (as per Dix-Carneiro and Kovak (2017)).

Thirdly, the paper demonstrates the inherent costs of discretising space when attempting to measure effects that are continuous in nature. The use of discrete geographic units is near ubiquitous in spatial variation analysis, and yet is not an innocuous decision. Estimated results of zero may simply be because the spatial discretisation is at the wrong scale – in either direction – rather than due to genuinely absent effects. In short, the treatment of space is not a mere technicality but fundamental to the outcomes of interest. This lesson applies to a broader range of applications than just employment or agglomeration spillovers. Any research question using spatial variation must consider the issue carefully.

Lastly, the paper also provides an initial look into the spatial scale and strength of possible spillover mechanisms. I find evidence that supports the relevance of

[4]For example, consumer and producer amenities in cities (Ahlfeldt et al., 2015), job search and commuting behaviour (Manning and Petrongolo, 2018; Hassink and Meekes, 2018), and export learning behaviour (Kamal and Sundaram, 2016; Bisztray et al., 2018) among others.

local product demand spillovers as a possible transmission mechanism.

The paper connects three strands of literature. Firstly, there is a large body of literature on regional dynamics following localised shocks. This paper extends the literature by looking at the micro level, in particular at the behaviour of individual firms and highly detailed spatial scales. Traditionally, the broader regional dynamics literature has followed the spatial equilibrium adjustment framework whereby regional outcomes converge overtime following shocks (surveyed in Moretti (2011)). A recent subset of the literature calls the validity of regional convergence into question. Papers have focussed on the strong persistence of shocks and, in particular, how the effects of shocks appear to exacerbate rather than mitigate overtime.[5] Amior and Manning (2018) argue that the deviation from spatial equilibrium adjustment is due to serial correlation in the labour demand shocks - in effect what may initially be temporary shocks are converted into persistent shocks. In a similar vein, Dix-Carneiro and Kovak (2017) show that the response of Brazilian regions exposed to trade shocks is twice as strong ten years after the shock than five years after the shock. They suggest that sluggish adjustment at the firm level interacting with local agglomeration forces may be generating the required serial correlation. However, given the regionally aggregated data, they are not able to test their firm level hypothesis.

Secondly, the paper extends the agglomeration literature with dynamic, spatial analysis of negative employment shock spillovers across a broad range of firms and industries. Much of the agglomeration literature is focussed on the difficulty of identifying static agglomeration spillovers, as clean sources of exogenous variation approaches are difficult to come by (Moretti, 2011). Some use narrow policy discontinuities in a particular subset of areas or firms.[6] These have the advantage of clean event study approaches, but often lack external validity to other areas, policy designs or industries. At the other end of the spectrum, others use broad Bartik instrument approaches - they cover a wide range of possible shocks but are subject to well known identification concerns (Goldsmith-Pinkham et al., 2018). Gathmann et al. (2017) and others take a middle-ground approach, by pooling a wide-range of masslayoffs into multiple event studies. Gathmann et al. (2017) find substantial spillovers from masslayoffs at the German regional level. It is this identification approach that I follow here.

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[5]Recent empirical research on regional dynamics following labour shocks include Topalova (2010), Autor et al. (2013), Kovak (2013), Dao et al. (2014), Hakobyan and McLaren (2016) and Monte et al. (2018)

[6]Example research designs include using regions around the cutoff for regional funding grants and tax subsidies, or a new large plant. See for example Devereux et al. (2007) and Busso et al. (2013)

As well as measuring the magnitude of spillovers, the literature also concerns itself theoretically and empirically on possible sources of agglomeration. Since Marshall (1890), economists have identified the theoretical importance of labour market risk pooling, input-output linkages and knowledge spillovers as agglomeration sources. There is an amount of empirical work demonstrating that these agglomeration forces generate benefits to firms from the co-location of other firms.[7]. Conversely, these generate a negative impact on firms when nearby firms reduce operations, as quantified by Helm (2017) for German local employment shocks. Again, however, the data and empirical strategies used, including for Helm (2017) are aggregated so analysis is restricted to region-industry level.

The third literature stream focuses on the degree of localisation and the spatial scale involved in economic forces. As yet, detailed questions about the spatial scale and suitable analysis methods have not been applied to spillover effects of large, negative employment events. Micro level effects of other processes as diverse as job search, commuting, export learning behaviour, production spillovers and local consumption amenities are often found to be highly localised.[8] Such papers also explore the heterogeneity in spatial scales, in recognition that market size or spatial reach should not be imposed as constant across all applications. For example, Hassink and Meekes (2018) estimates a wide variety in local labour market sizes based on skill, gender and other individual characteristics. To analyse spatial scales in detail, the literature is required to use much more detailed geographic information and more complex analysis methods that move towards a continuous treatment of space (Manning and Petrongolo (2018) and Ahlfeldt et al. (2015) provide two examples).

The rest of the paper proceeds as follows. Section 2.2 outlines the conceptual framework, in particular the treatment of space as a continuous concept, and the empirical strategy and data used. Section 2.3 presents the empirical results of the analysis of spillover spatial patterns on impact. Building on the impact results, Section 2.4 outlines the costs of more standard analysis methods by comparing the spillover spatial patterns with those using discretised geographic units. Next, Section 2.5 presents the analysis of the dynamic responses to mass layoffs. Section 2.6 evaluates possible spillover channels and Section 2.7 concludes with a discussion.

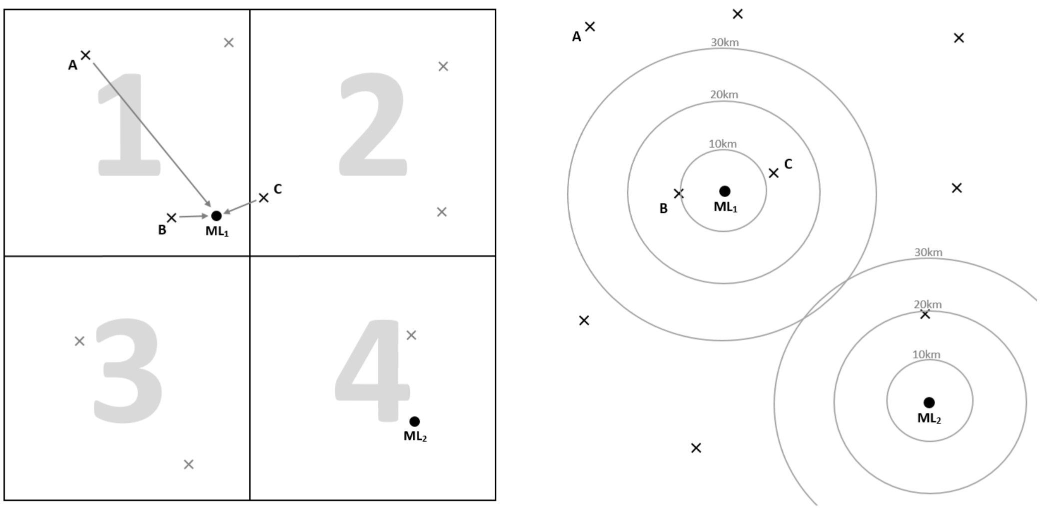
[7]See for example Harhoff (1999), Devereux et al. (2004) and Graham et al. (2009)

[8]Example work includes Manning and Petrongolo (2018); Hassink and Meekes (2018); Ahlfeldt et al. (2015); Kamal and Sundaram (2016); Bisztray et al. (2018). Spillovers often cascade into other areas so the macro effects may operate on larger spatial scales.

2.2 Framework and empirical strategy

The conceptual and empirical framework of the paper is based on treating space as continuous. Traditionally, analysis has discretised space by dividing areas up into mutually exclusive and exhaustive units. These may be arbitrary from an economic point of view (e.g. politically demarcated administrative regions) or have some economic principles defining them (e.g. commuting zones where a certain fraction of the population both reside and work in).[9] Either way, what is naturally a continuous concept is divided into indepedent units.

The most common approach to empirical analysis of local economies is quasi-experimental analysis of these discretised areas. For example, difference-in-differences can be used to compare discrete units that have experienced the event in question (e.g. a masslayoff, a rise in the minimum wage) to those that have not.



(a) Discretised space **(b)** Continuous space

Figure 2.1: Measurement of spillovers across space

Panel A of Figure 2.1 demonstrates this approach using the issue of masslayoff spillovers. Firms, denoted by a cross, are scattered across a space which is divided up into four regions. Regions 1 and 4 on the diagonal experience a masslayoff (ML) while regions 2 and 3 on the off-diagonal do not. The outcomes of firms located in regions 1 and 4 are compared to firms in regions 2 and 3.

[9]It should be noted that there are substantial linkages between any commonly used local labour market definition. For example, there are 320 travel to work areas (TTWAs) in the UK defined as having at least 75% of the working residents work in the area and at least 75% of the workers also live in the area. That means up to 25% flow across the boundaries for work every day. These cannot be thought of fully separate geographic entities in any sense. Manning and Petrongolo (2018) further discusses the limitations of discrete local labour market measures.

Such an approach is prone to bias. Firstly, if the economic forces of interest operate on a much more local scale than the unit of analysis, it is likely that they will be undermeasured and potentially not identified at all. For example, if only firms very close to the masslayoff (e.g. Firm B) are affected, while further away firms (e.g. Firm A) are not, the estimate will provide some average of the two. Implicitly, all firms within region 1 are assumed to be equally treated.

Secondly, the approach assumes that the spillover effects do not cross the unit borders. All firms within regions 2 and 3 are assumed to be entirely unaffected by the masslayoffs. This will be violated if an event occurs near the border (e.g. firm C is affected by the first masslayoff) or if the spillovers operate on a larger scale than the geographic units used. In either case, regions 2 and 3 cannot be used as 'controls' in empirical analysis.

The approach here avoids artificially discretising space. As I have individual firm locations, I am able to retain the spatial structure. I can then use the masslayoff as the centre of the treatment, and analyse how firms of varying distances are affected. Panel B of Figure 2.1 demonstrates this approach. This approach allows for Firms B and C to be approximately equally affected by the first masslayoff, and Firm A to be unaffected.

2.2.1 The spillover distance function

The analysis uses masslayoffs as a localised employment shock. A masslayoff will generate spillovers to nearby firms if a firm's productivity is related to local economic activity. Phrased differently, in the presence of local agglomeration forces, a decrease in local employment will reduce the productivity, hence output and employment, of other nearby firms.

Consider a simple price taking, profit maximising firm i with Cobb-Douglas production choosing their optimal level of inputs. In the short run, capital, $K = \bar{K}$ is fixed and only labour L is chosen:

$$\max_{L} \left\{ A_i L^{\alpha} \bar{K}^{\beta} - wL - r\bar{K} \right\} \tag{2.1}$$

Wages are denoted w, capital prices r, and output prices are normalised to $p = 1$. Labour is chosen such that the marginal product of labour equals the prevailing wage $w = MPL = \alpha A_i L^{\alpha-1} \bar{K}^{\beta}$. The optimal level of labour, in logs, is therefore:

$$\ln L^* = \underbrace{\frac{1}{1-\alpha}\ln A_i}_{\text{object of interest}} + \frac{1}{1-\alpha}\big[\ln\alpha + \beta\ln\bar{K} - \ln w\big] \tag{2.2}$$

Spillovers occur when a firm's log productivity $\ln A_i$ is a function of local employment - this empirical fact is well documented.[10] The analysis here is particularly interested in whether $\ln A_i$ is a function of distance-weighted employment, and in particular what that distance function is.

Consider firm productivity as a function of distance-weighted local employment, and all other productivity characteristics unrelated to distance $\bar{A}_i$: $A_i = \bar{A}_i e^{f(\text{distance weighted employment})}$. I assume the distance weighted local employment function sums across all nearby firms, j. Inside the sum, the contribution of nearby firm j to firm i's productivity is some function, f, of the distance between i and j, d_{ij}, multiplied by some function, g, of j's employment size E_j.

$$\ln A_i = \ln\bar{A}_i + \sum_j f(d_{ij}).g(E_j) \tag{2.3}$$

The current set up places no restriction on the function form of the distance function to each nearby firm - and this distance function is the key function of interest. The direct impact of the employment size of j is not constrained. However, the set up does assume that the impact of distance is multiplicatively separable to the employment size effect.

A masslayoff is a large change to the employment of some nearby firm, k. This will affect the productivity of nearby firm i:

$$\Delta\ln A_i = f(d_{ik}).\underbrace{\Delta g(E_k)}_{\text{masslayoff}=1} + \sum_{j\neq k} f(d_{ij}).\Delta g(E_j) \tag{2.4}$$

I can use the variation in distance to this masslayoff to identify the distance function. As productivity itself is not observable, I revert back to the observed short run employment change of firm i. Combining equation 2.4 with equation 2.2, I get a more estimable equation for short run log employment changes:

$$\Delta\ln L_i = \underbrace{f(d_{ik}).\text{masslayoff}_k}_{\text{masslayoff distance effect}} + \underbrace{\sum_{j\neq k}\frac{1}{1-\alpha}f(d_{ij}).\Delta g(E_j)}_{\text{other local employment changes}} - \underbrace{\frac{1}{1-\alpha}\Delta\ln w}_{\text{local wage adjustment}} \tag{2.5}$$

[10]The spillovers and agglomeration literature discussed earlier documents the empirical evidence.

$g(E_k)$ is not of direct interest here, so $\Delta g(E_k) * \frac{1}{1-\alpha}$ is replaced with a binary variable for a masslayoff in nearby firm k.

2.2.2 Empirical strategy

The analysis uses an event study approach based around localised mass layoffs. As stated in more detail in Section 2.2.3, a masslayoff is defined as a single plant losing 1000 or more workers in a year which is not associated with regularly fluctuating employment or an ownership change. The masslayoff provides a shock to nearby employment for all local firms.

I follow the extensive masslayoff literature with the identifying assumption that large employment losses are unrelated to local factors, hence are exogenous to the local area. They are assumed to be driven by national or international forces such as trade shocks or industrial decline. Non-tradeable, locally consumed goods that respond to local events are, in general, produced by firms too small to generate a 1,000 person masslayoff. Such events are usually restricted to tradeable services, manufacturing etc. The local exogeneity justification is a very standard chain of logic, but one that does not come without its critics.

In this context, I have the added advantage of highly detailed firm level microdata; much of the literature relies on aggregated data. These data mean I am able to control for annual industry shocks up to the five digit level, leaving only sub-national industry variation for all non-masslayoff firms. This removes concern that other firms in the local area are laying off staff in response to the same industry trend that generated the masslayoff. I am also able to control for general local shocks, which could include local policy shifts or broad spending shocks to hit the local economy. What remains is therefore sub-national, local industry variation, primarily in distance to the masslayoff event.

Given the masslayoff event construction, I then construct distance measures for each firm to the masslayoffs. As a result, each firm is linked to their closest masslayoffs and key data on those masslayoffs, such as industry and employment size.

I then estimate the relationship between employment changes in each non-masslayoff firm and distance to the nearby masslayoffs, considering in turn contemporaneous masslayoffs and masslayoffs that occured in the previous years. Figure 2.2 demonstrates the timing of the event study approach. Our dependent variable is some measure of employment change for firm i (local plant) between time $t-1$ and t. The event generating the shock is the closest masslayoff in the period of interest. A contemporaneous (lag 0) event would be a masslayoff that also occurs

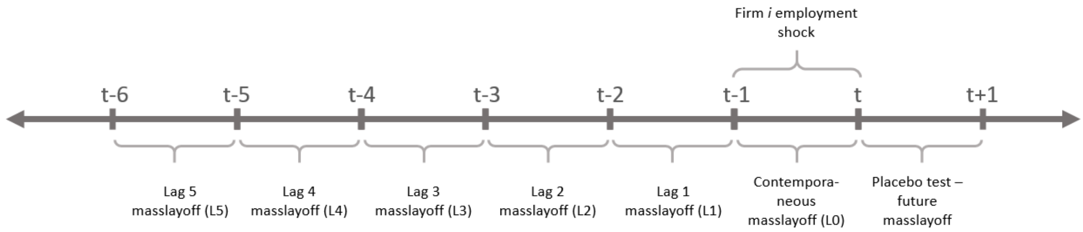


Figure 2.2: Timing of event study estimation strategy

between $t-1$ and t: i.e. a local firm sheds at least 1,000 workers in the time period. Lagged masslayoffs occur in preceeding time periods, as demonstrated in the figure (2.2).

Using the event study approach and motivated by equation 2.5, the baseline estimating equation is of the following form:

$$\Delta \ln L_{ijlt} = \alpha + \beta f(d_{ik}).\text{masslayoff}_k + \gamma X_{ijlt} + c_j + c_t + c_l + \epsilon_{ijlt} \tag{2.6}$$

Where $\Delta \ln L_{ijrt}$ is the change in log employment at firm i in industry j at location l between time t and time $t-1$ and, as standard, can be approximated as percentage changes. For firms that shut down over the period, $\Delta \ln L_{ijrt}$ is simply $-\ln L_{ijr,t-1}$.[11] $f(dist_{ik})$ is a function of the distance to the closest masslayoff, k. X_{ijlt} are relevant factors or controls such as the number of masslayoffs within a certain distance during the year in question, or the distance of the second closest masslayoff. A variety of fixed effects are used, including industry, time and location, as well as industry-time and location-time fixed effects. Errors are primarily clustered at the two digit industry level, although a variety of spatial and industry-spatial errors structures are investigated.

The primary features of interest are the functional form of distance, $f(dist_{ik})$, and the strength and direction of the effects, β. Instead of imposing functional forms on f, the baseline results take a non-parametric approach to estimating the distance function. f is estimated using a collection of mutually exclusive dummy variables of distance to the closest masslayoff, for example:

[11] Numerically, this is equivalent to the firm shedding all but its final employee. This is a reasonable approximation of log employment loss for larger firms. As we see later, smaller firms on whom the approximation might make a material impact are not the primary drivers of the result. Therefore the assumption is unlikely to be particularly influential.

$$\begin{aligned}\Delta \ln L_{ijlt} =& \alpha + \beta_0 dist_{ik}^{0-1} + \beta_1 dist_{ik}^{1-2} + \beta_2 dist_{ik}^{2-3} + \beta_3 dist_{ik}^{3-4} + \beta_4 dist_{ik}^{4-5} + \\ & \beta_5 dist_{ik}^{5-10} + \beta_{10} dist_{ik}^{10-20} + \beta_{20} dist_{ik}^{20-40} + \gamma X_{ijlt} + c_j + c_t + c_l + \epsilon_{ijlt}\end{aligned} \tag{2.7}$$

The dummy variable $dist_{ik}^{x-y}$ equals 1 if the distance from firm i to the closest masslayoff k lies between x and y kilometers. For firms who are not located near any masslayoff - i.e. the closest masslayoff is further than the maximum distance dummy - will have all dummy variables equal to zero. They are therefore the baseline case, and the β coefficients $(\beta_0, \beta_1, \beta_2, \beta_3, \beta_4, \beta_5, \beta_{10}, \beta_{20})$ plot non-parametric estimates of the distance function.

It should be noted that two key terms in equation 2.5 are not included in the estimating equations, 2.6 and 2.7. Firstly, the adjustment of local wages will in part determine firm i's short run employment adjustment. Local wages are notoriously downwardly rigid empirically, but nonetheless analysis in section 2.3 confirms their zero response here.

Secondly, the responses of other non-masslayoff firms in the local area are not included. Undoubtedly, they too will respond directly to the masslayoff, and their responses will generate a standard reflection issue. The β coefficients should therefore be interpreted as a sum of firm i's direct adjustment to the closest masslayoff, and the indirect adjustments to other nearby firms' own adjustments to the masslayoff. The inability to separate direct and indirect effects is a matter of results interpretation, rather than fundamental to the approach. Provided the masslayoff event itself is exogenous to the local area (the earlier identifying assumption), other sources of local firm changes should be independent of the masslayoff and not cause endogeneity problems.

Appendix B.2 outlines the variety of robustness check undertaken. Standard event study checks such as a placebo check for anticipation effects are included. Setting specific concerns around spatial sorting, the Global Financial Crisis and spatial clustering of multiple masslayoffs, among others, are also addressed.

2.2.3 Data

The primary dataset used for analysis is the annual UK Business Structure Database (BSD) from 1997-2017. The BSD encompasses almost the entire universe of business entities in the UK, encorporating 99% of economic activity in the UK. All firms in the UK who employ at least one staff member registered for PAYE tax collection and/or are eligible for Value Added Tax (VAT) are included in the BSD. The BSD is available at both the parent company level (Enterprise Unit - EU) and local plant level (Local Unit - LU). Each level provides birth and death dates, tax information, five digit industry codes and employment counts. The EU level dataset provides turnover of the company. I use the LU, plant level information as the topics of concern relate to highly localised employment.

Crucially, extremely detailed location information down to the postcode level is available for both the EU and LU. There are around 1.8million postcodes in the UK for a population of approximately 66 million, with the average postcode covering five properties.[12] Postcodes therefore provide an almost exact pin-point location. The postcodes are mapped to northings and eastings using the Office for National Statistics Postcode Database, resulting in precise coordinates matched to every plant and firm in the UK.

I exclude public sector employment entities and retain all companies, sole proprietors and partnerships in the BSD. Due to the requirement for VAT registration and/or at least one PAYE enrolled employee, the smallest of sole proprietors are not included in the dataset. What remains is effectively the near-entirety of the UK private sector, with an average of X plant observations annually totalling Y observations overall.

I then define and identify every local mass layoff event in the UK from 1997-2017. These are defined as a plant shedding at least 1000 net employees in a given year. This can occur through either a plant of 1000 or more employees shutting down or a larger plant firing 1000 workers and remaining active with a smaller employment count. To avoid capturing seasonal workers or other types of highly fluctuating employment, I exclude those firms which rehire 1000 workers in the subsequent few years, and those who hire 1000 workers in the previous year. I also check that the plant has not simply changed ownership, name or ID code by using the demographic event information available in the dataset; a masslayoff through plant death has to coincide with the BSD labelling the event as death too (as opposed to merger, acquisition etc). Appendix B.1 presents key descriptive statistics for the firm and masslayoff data.

One issue with the construction of the masslayoff variable is that it also captures large, local employment outsourcing. For example, a large firm that restructures its labour force by shifting many support jobs to external providers will be included in the events. If the external providers are in close proximity geographically, this is not a local negative employment shock in any sense. Unfortunately, the

[12]Details from BPH postcodes https://www.bph-postcodes.co.uk/guidetopc.cgi

occurrence of such events cannot be measured with the data available. If such events are not negligible in number, they will provide an attenuation bias in the estimated treatment effect by mixing the pool of events into true treatments and false treatments.

In section 2.3, the adjustment of local wages are considered. For this, I use the UK Annual Survey of Hours and Earnings (ASHE), as the BSD does not contain the required wage information. The ASHE is a one percent sample of UK workers, based on national insurance (tax) numbers. It is compulsorily employer reported from payroll information so is considered highly accurate relative to workers' self-reported earnings. Sampled individuals are included in the dataset for each year they are employed, even if changing employers. From the ASHE, I construct an annual worker panel that includes hourly wages and employment postcode. For individuals with multiple jobs in any given year, I take their reported main job. The required postcode information is only reliably provided from 2004, so the panel spans 2004-2017.

2.3 Spatial distribution of effects on impact

2.3.1 Baseline

The baseline results follow equation 2.7 by regressing employment change at the non-masslayoff firm level on distance to the closest masslayoff in the same year. Collections of mutually exclusive dummy variables are used as the non-parametric distance function. These are labelled by the distance they refer to: for example, 'dist 2-3km' equals one if the closest masslayoff in the current year is between 2 and 3km from the plant in question. Industry (2 digit SIC), postcode (2 digit) and year dummies are included in each regression as fixed-effect controls. All standard errors are clustered at the two digit SIC industry level.

In all three specifications of Table 2.1 we find that plants located close to a masslayoff experience substantial employment loss, and this mitigates as the distance increases. The preferred specification is column three as it includes the most detail for the closer distances. The coefficients on the distance dummies in column three uncover strong but very localised employment spillovers. As we see, the employment loss rapidly decays within a few kilometers; the impact on firms between two and three kilometers from the event is approximately half that of those between one and two kilometers, which in turn is half that of those located within one kilometer. Employment losses become insignificant at approximately the twenty kilometer mark.

Table 2.1: Baseline estimates of spatial effects on impact

	(1) $\Delta log(L)$	(2) $\Delta log(L)$	(3) $\Delta log(L)$
dist 0-5km	-0.0235***		
	(0.00323)		
dist 0-2km		-0.0468***	
		(0.00500)	
dist 2-5km		-0.00973***	
		(0.00267)	
dist 0-1km			-0.0702***
			(0.00769)
dist 1-2km			-0.0266***
			(0.00362)
dist 2-3km			-0.0147***
			(0.00305)
dist 3-4km			-0.00813**
			(0.00269)
dist 4-5km			-0.00792**
			(0.00254)
dist 5-10km	-0.00641***	-0.00656***	-0.00683***
	(0.00178)	(0.00179)	(0.00179)
dist 10-20km	-0.00415**	-0.00407**	-0.00424***
	(0.00121)	(0.00121)	(0.00119)
dist 20-40km	-0.0000138	0.0000272	-0.0000507
	(0.00122)	(0.00123)	(0.00121)
Cons	-0.0434***	-0.0427***	-0.0426***
	(0.00642)	(0.00648)	(0.00641)
Controls	Yes	Yes	Yes
N	66,014,522	66,014,522	66,014,522
R^2	0.023	0.023	0.023

Standard errors in parentheses, clustered on 2 digit industry. Variables are a set of dummies indicating distance to closest masslayoff. Fixed effects are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

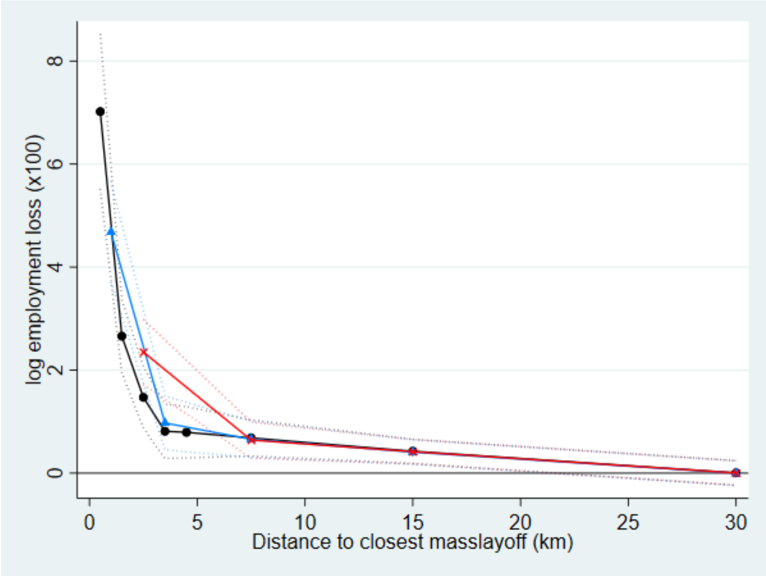


Figure 2.3: Employment loss estimates from Table 2.1, contemporaneous masslayoff

Figure 2.3 plots the coefficients from Table 2.1 along with 95 percent confidence intervals. Column three is plotted in black with circular points, column two is plotted in blue with triangular points and column one is plotted in red with cross points. The initial coefficient points are lower for the blue (Column 2) and red (Column 1) specifications as the less detailed dummies provide weighted estimates for the smaller distances.

Figure 2.4 maps the predicted employment impacts of a hypothetical masslayoff centred in Cambridge, UK, using the decay estimates from the baseline regressions.[13] The map is a visual display of the high degree of localisation. Cambridge is a very small city with a dense population of around 120,000 individuals. The substantial employment spillovers would only be experienced by a subset of the city area, before rapidly decaying further out.

These non-parametric estimates accurately demonstrate the magnitude and speed at which localised spillovers propagate spatially. They suggest that employment spillovers are highly localised, affecting very close firms most strongly. At least at the local firm level, they become negligible at fairly conservative distances, approximately ten kilometers.

The rapid spatial decay patterns have several implications. In terms of measurement, they mean that firm-level spillovers are highly localised. Standard

[13]The masslayoff is centred over the Faculty of Economics which is located next to the Faculty of Law at the University of Cambridge. The hypothetical scenario is therefore at least 1,000 economists (and/or lawyers) being laid off. This may well be a net positive boost to the local Cambridge economy, however I abstract from such a discussion by imposing the baseline estimates.

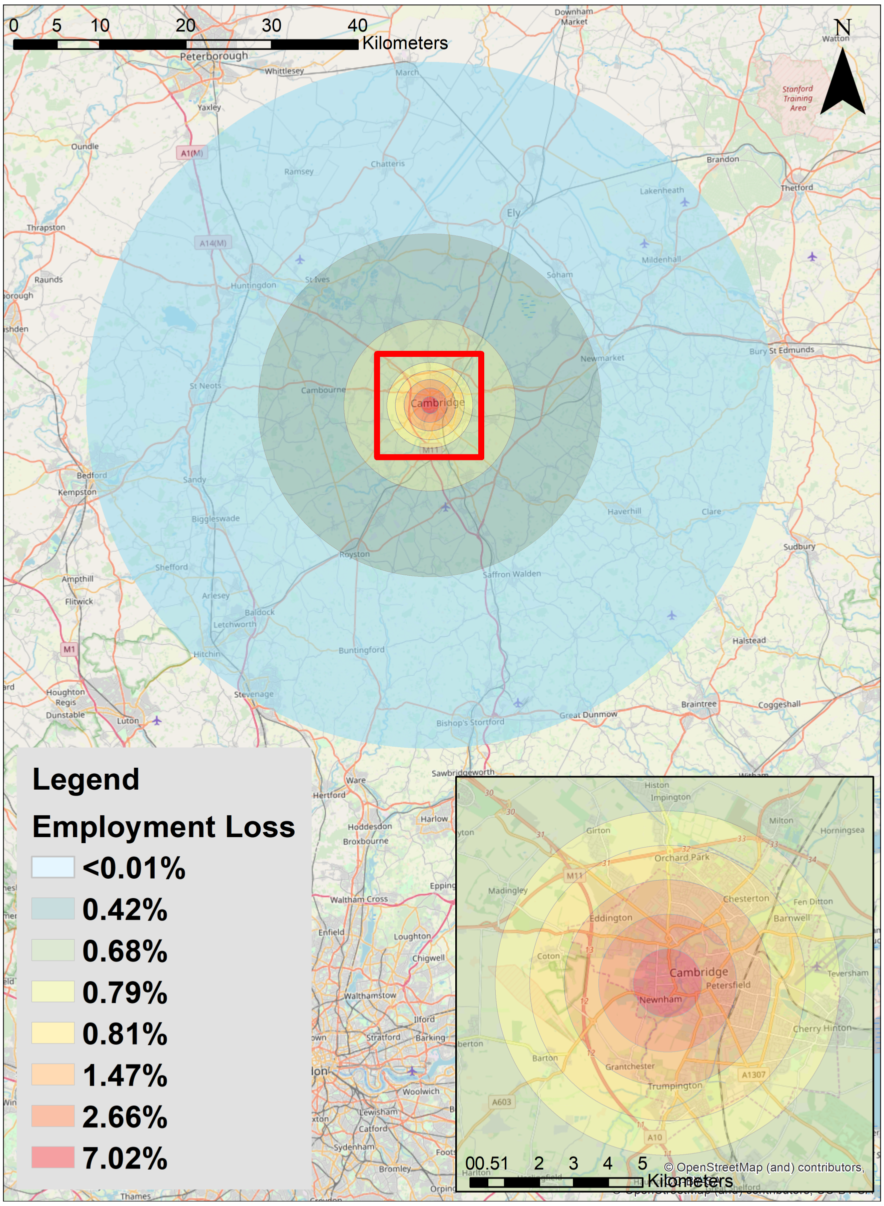


Figure 2.4: Map of predicted employment impacts from a hypothetical masslayoff in Cambridge, UK

estimation strategies relying on larger administrative areas may therefore fail to pick them up. As the more geographically aggregated specifications in Figure 2.3 show, larger geographic units provide a weighted average of the spillovers within the area. Averaging across an administrative area ten or twenty kilometers around a masslayoff would provide very small, potentially insignificant results given the coefficients above.

Economically, the results also show that the firm-to-firm transmission mechanisms must be highly localised in nature. A high degree of localisation is consistent with much of the urban economics agglomeration literature - many economic linkages from productivity spillovers to consumption amenity spillovers appear to operate in a very localised way.[14] The degree of localisation in these employment spillovers can help elucidate the mechanisms at hand. Many have been discussed hypothetically in the literature, and these results give more weight to those that operate very locally around the masslayoff event. Section 2.6 continues down this chain of logic further.

2.3.2 Impact on local wages

The baseline analysis has so far ignored impacts on local wages; implicitly I have ignored price adjustments in favour of quantity adjustments for labour. As touched on in Section 2.2, equation 2.5 demonstrates that local employment adjustment will partly depend on the response of local wages to a masslayoff. One might expect that a large reduction in local labour demand will depress local wages. Wage adjustments exclusion from the picture has so far been driven by the extensive literature finding strongly downwardly rigid wages.[15]

However, the same logic applied to the employment spillover analysis can be applied to the local wage phenomenon. Perhaps existing results that find no effect on local wages are driven by a mis-specification of distance. Wage analysis will also suffer from the costs of discretising space, and the zero results may derive from discretisation rather than genuinely zero effects.

I estimate the impact of a masslayoff on local wages using the same methodology as for employment adjustments. The ASHE worker panel provides the required hourly wage information. I use both hourly wages excluding overtime and hourly wages including overtime to allow for possible impacts through changes in overtime employment. The change in the individual worker level log wage, $\Delta \ln(w_{it})$, is regressed on the distance to the closest masslayoff.[16] Occupation, location and year dummy control variables are included to keep the analysis consistent with the firm level employment analysis. Standard errors are clustered at the 2-digit occupation level.

Table 2.2 displays the estimated wage impacts. Column 1 uses hourly wages

[14] See for example Ahlfeldt et al. (2015), Manning and Petrongolo (2018) and Hassink and Meekes (2018).

[15] See Bewley (2009), Kahn (1997), Altonji and Devereux (1999) and Elsby (2009) for general analysis of the downwardly rigid wage phenomenon. Kaur (2019) and de Ridder and Pfajfar (2017) address downwardly rigid wages at the local level.

[16] Recorded changes will include an individual receiving a pay change from their existing firm, and individuals switching firms. For individuals laid off, their change in log wages is recorded as $-\ln(w_{i,t-1})$. For workers who have just entered employment, their change in log wages is recorded as $\ln(w_{it})$

Table 2.2: Wage changes as a function of the distance to the closest masslayoff

	(1) $\Delta log(W_1)$	(2) $\Delta log(W_2)$
dist 0-1km	0.0862	0.0861
	(0.0912)	(0.0913)
dist 1-2km	0.00521	0.00532
	(0.0192)	(0.0190)
dist 2-3km	-0.00133	-0.00174
	(0.0224)	(0.0223)
dist 3-4km	-0.0180	-0.0182
	(0.0199)	(0.0198)
dist 4-5km	-0.0162	-0.0160
	(0.0167)	(0.0166)
dist 5-10km	0.0161	0.0159
	(0.0144)	(0.0144)
dist 10-20km	0.00632	0.00613
	(0.0103)	(0.0103)
dist 20-40km	-0.00694	-0.00693
	(0.00799)	(0.00799)
Cons	6.826^{***}	6.818^{***}
	(0.0992)	(0.0994)
Controls	Yes	Yes
N	2,256,208	2,256,021
R^2	0.148	0.147

Standard errors in parentheses, clustered on 2 digit occupation. Variables are a set of dummies indicating distance to closest masslayoff. Controls are 2 digit occupation codes, 2 digit postcode, and year fixed effects. Years included are 2004-2017. Column 1 uses hourly wages excluding overtime, W_1, Column 2 uses hourly wages including overtime, W_2. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

excluding overtime, denoted W_1, and Column 2 uses hourly wages including overtime, denoted W_2. As can be observed, at no distance are there significant effects on wages from masslayoffs. The results here confirm the presence of strong downward rigidities in wages. It appears that employment adjustment following localised shocks occurs through the quantity of labour employed rather than the price of labour.

2.3.3 Heterogeneity by firm type and location

I investigate heterogeneity by firm and location observables. These help deepen the picture about features of the spatial spillovers and help assuage concerns that the results are purely down to detailed spatial sorting and associated spatial autocorrelation in shocks.

A natural starting point is to identify which firms are driving the spillover results. Are smaller firms located near to mass layoffs more susceptible to employment spillovers, or are their larger counterparts the primary drivers of spillover employment loss?

To do so, I segment the sample into larger and smaller non-mass layoff firms using an employment cut-off of either 20 or 50 employees. I repeat the baseline regressions on each sample in turn. I also pool the entire firm sample and include the firm size variables both as a level term and as an interaction with the distance function. Both approaches shed light on whether small or larger firms receive stronger spillovers, and what the distance decay function for each is.

The results, displayed in Table B.6 in Appendix B.3, demonstrate that all firm sizes experience employment spillovers if located in close proximity to a masslayoff and all firm sizes exhibit a similar spatial decay rate. However, larger firms (those with more than 20 or 50) employees do have proportionately stronger responses. It appears that small firms respond less strongly but still exhibit employment losses.

I next evaluate the specific role of the manufacturing industry. The advantage of the data and empirical approach used here are that they are able to capture a broader range of industries. However, much of the existing local spillovers literature has restricted focus to the manufacturing sector so it is of value to see how important the distinction is.

I broadly classify a firm (including a masslayoff firm) as in the manufacturing sector if their 2 digit SIC codes are in the range 15-34 (based on the UK Data Service SIC coding). The analysis that follows is in two parts; assessing the role of a manufacturing mass layoff event (manufacturing is the 'generating' firm) and assessing the response of nearby non-mass layoff manufacturing firms to any mass layoff event (manufacturing is the ‘receiving’ firm).

Results are similar to the firm size results and are displayed in Table B.7 in Appendix B.3. Manufacturing firms incur larger employment losses from being located very near (any) masslayoff. Non-manufacturing firms still have a loss function that decays, but it is smaller in magnitude across all distances. This may be inextricably tied up with the firm size results. Manufacturing firms tend to be larger and as larger firms have larger losses (even proportionately) it is unclear whether the key driver is firm size, manufacturing or some omitted covariate.

Whether or not the masslayoff itself is from a manufacturing plant does not seem to meaningfully affect the spatial spillovers. Segmenting the sample into firms located closest to a manufacturing masslayoff and those located closest to a non-manufacturing masslayoff give similar results. If anything, the non-manufacturing masslayoffs seem to generate slightly stronger spillover results.

I also consider whether a location’s employment density matters for the observed spillovers. If a firm is located in a dense area economically, the presence of a lot of additional economic activity may either amplify or mitigate the shock. The latter would imply that density provides some insurance value against shocks to a single large nearby employer.

The economic density each non-masslayoff firm’s location is measured at the employment per square kilometer of a 3km by 3km grid around each firm. For meaningful interpretation, this employment density measure is standardised. Results, displayed in Tables B.8 and B.9 in Appendix B.3 suggest that a one standard deviation increase in employment density has a very small level effect on employment changes overall. However, there is a positive interaction term with the distance function, suggesting that density somewhat mitigates the firm level employment loss associated with locating very near a masslayoff. The insurance value this provides is in the order of two percentage points less employment loss for a firm located immediately next to a masslayoff.

Of course, these results are all for the individual firm’s employment loss. There may be less employment loss at the firm level in a dense environment, but a denser environment is associated with more firms. Once aggregating over all nearby firms, it may still be the case that denser areas lose more employment overall from a masslayoff, even if individual firms are somewhat insured.

2.4 The costs of discretising space versus a continuous measure

The analysis so far has used continuous measures of distance to analyse the spatial scale of masslayoff employment spillovers. The approach used is novel in part because it relies on precise location data that are rarely available.

The more standard method for analysing spatial features is to discretise space into mutually exclusive and exhaustive units. A difference-in-differences style approach is typically used to compare those units that have experienced an event to those that have not. As discussed in Section 2.2, such an approach will suffer from two potential sources of bias. If the area affected by the spillovers exceeds the geographic unit size used, control units will become contaminated. If the spillover area is much smaller than the geographic unit size used, estimated results will average out close to zero. Both can cause estimates that are biased towards zero.

To demonstrate these costs more fully, I now shift to estimating spillovers using discrete spatial methods. I begin by dividing the space spanned by the UK landmass into grids of set unit sizes. In effect, I take the approach displayed in Panel a) of Figure 2.1. The smallest discretisation of space divides the entire UK up into 1km by 1km grid squares. The largest divides the UK up into 40km by 40km grid squares. A range of intermediate grid sizes are constructed as well.

For each grid square (of a given size), I sum up the number of masslayoffs occuring in the grid square in the year in question. This equals zero for squares with no masslayoff - the majority - and one for those that include a masslayoff.[17] I then regress the change in firm level log employment on the presence of a masslayoff in the firm's grid square. This is standard difference-in-differences; comparing the change of a firm in a unit that has experienced a masslayoff to the change of a firm in a unit that has not. For consistency with the continuous results, the same controls are used (controls for industry, year fixed effects and two digit postcode fixed effects) and standard errors remain clustered at the 2 digit industry level.

It should be noted that the grid square sizes are not directly comparable to the analogous distance dummies in the continuous analysis. The continuous analysis effectively draws concentric circles centred around the masslayoff. The discrete

[17] Very occasionally, more than one masslayoff occurs within a grid square in a given year. This is slightly more common when larger units (e.g. 30-40km are used). As a robustness check, I also turn the masslayoff count into an indicator variable that equals zero if no masslayoff occurs, and one if one or more occurs.

Table 2.3: Discretising space into units: count of masslayoffs in unit

Unit size	(1) $\Delta log(L)$ 1x1km	(2) $\Delta log(L)$ 2x2km	(3) $\Delta log(L)$ 3x3km	(4) $\Delta log(L)$ 4x4km	(5) $\Delta log(L)$ 5x5km
# ML in unit	-0.0249***	-0.00935***	-0.00589***	-0.00449***	-0.00298***
	(0.00392)	(0.00251)	(0.00119)	(0.000893)	(0.000523)
Constant	-0.0476***	-0.0475***	-0.0471***	-0.0469***	-0.0468***
	(0.00662)	(0.00657)	(0.00659)	(0.00661)	(0.00664)
Controls	Yes	Yes	Yes	Yes	Yes
N	66,014,522	66,014,522	66,014,522	66,014,522	66,014,522
R^2	0.023	0.023	0.023	0.023	0.023

Unit size	(6) $\Delta log(L)$ 10x10km	(7) $\Delta log(L)$ 15x15km	(8) $\Delta log(L)$ 20x20km	(9) $\Delta log(L)$ 25x25km	(10) $\Delta log(L)$ 30x30km
# ML in unit	-0.00112**	-0.000758**	-0.000124	-0.000148	0.000101
	(0.000329)	(0.000248)	(0.000223)	(0.000203)	(0.000173)
Constant	-0.0471***	-0.0472***	-0.0478***	-0.0477***	-0.0483***
	(0.00662)	(0.00664)	(0.00660)	(0.00660)	(0.00662)
Controls	Yes	Yes	Yes	Yes	Yes
N	66,014,522	66,014,522	66,014,522	66,014,522	66,014,522
R^2	0.022	0.022	0.022	0.022	0.022

Standard errors in parentheses, clustered at 2 digit industry level. Controls are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

approach says nothing about where in the grid square the masslayoff occurs, which is an inherent problem in the method. If the masslayoff is directly centred in the square of size xx$x$km, other firms in the same grid square will be located anywhere between 0 and $\sqrt{(\frac{x}{2})^2 + (\frac{x}{2})^2}$ km from the masslayoff. But if the masslayoff is near the corner of the grid square, a firm could be located up to $\sqrt{x^2 + x^2}$km from the masslayoff. Naturally, a firm located immediately across the border in an adjacent grid square will be closer.

The results illuminate the discretisation costs. For grid squares of very small sizes (e.g. 1x1km, 2x2km), significant effects on firms within the same unit are found. However, the magnitudes are much smaller than the continuous methods would suggest because the effects 'spillover' into nearby units, contaminating the controls. As the grid size is increased, the magnitude of the effects declines rapidly. Once grid sizes of around 10-20km are used, the point estimates are tiny and statistically insignificant.

In effect, estimates of the spillovers to nearby firms are zero. As the continuous method shows, strong spillovers are in operation and will be experienced by many firms within the 'treated' grid square. However, averaging across too large an area means they fail to be picked up by the difference-in-differences methodology.

Interestingly, the point at which the results become statistically insignificant - around 10x10km or 20x20km - is comparable in size to many administrative units or commuting zone units used in typical analyses.

2.5 Dynamics in the spatial distribution of effects

One of the motivations for the analysis is the (recently documented) phenomenon that very local shocks tend to generate larger impacts overtime as the effects strengthen. The classic spatial equilibrium adjustment process whereby the impact of shocks dissipates overtime is called into question. This section considers the dynamic effects at the firm level. In particular, I investigate the magnitude of the current firm level responses to masslayoff shocks that occurred in the past, and the spatial distribution of these.

To do so, I repeat the analysis of Section 2.3 using lagged masslayoff events. For each firm in time t, I calculate their change in log employment since $t-1$. I estimate a relationship between this current employment change and proximity to a masslayoff occurring at some $t-s$, where $s > 1$. The results displayed here are for the non-parametric distance function approach of equation 2.7, using a mutually exclusive set of dummies for the distance to the closest (lagged) masslayoff. The distance is the euclidean distance in kilometers between firm i (at time t) and the closest masslayoff at time $t-s$. Again, time, industry and postcode fixed effects are included, and errors are clustered at the 2 digit industry level. The full regression estimates are presented in Appendix B.4. Figure 2.5 summarises the distance coefficients for one to five lags of masslayoffs, and includes the contemporaneous results from Table 2.1 as T0 for comparison.

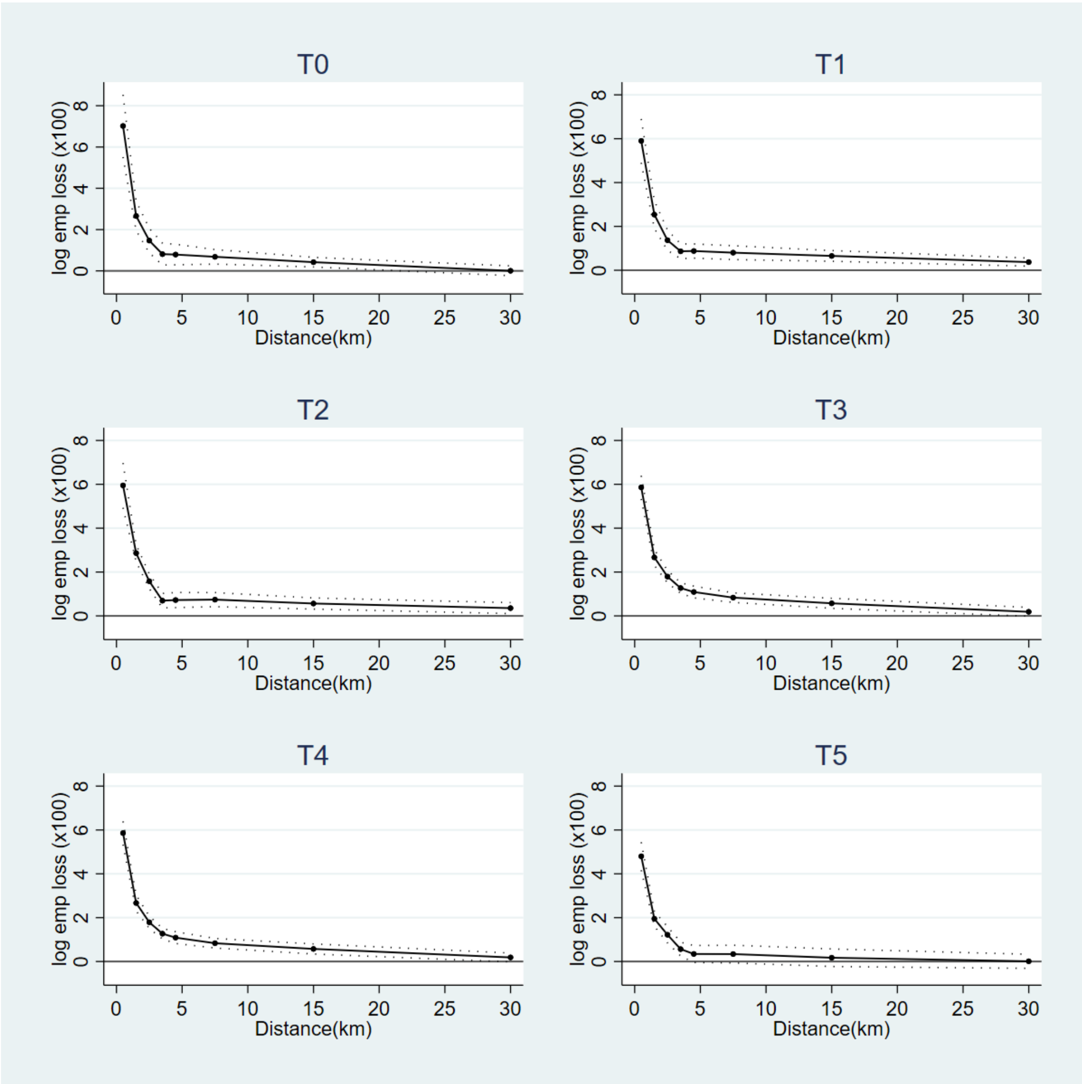


Figure 2.5: Year-to-year employment changes by distance to closest masslayoff in T0

As we see, significant negative employments impacts are present for firms located close to a masslayoff event that occurred for all lags. The magnitude does decline from the initial shock (T0) to the firm level impacts five years later (T5). The spatial decay patterns are also remarkably persistent. The meaningful impacts occur for firms within 5km of the event, and particularly so for those within one or two kilometers.

A few points should be considered when interpreting these results. Each set of time results is for an annual firm level change ($t-1$ to t) and the proximity to an event that occurred in the past ($t-s$, with $1 < s < 5$). Some firms will therefore have been born since the event, particularly for the further back lagged events. This is not conceptually problematic: the current impact on firms of a lagged event is the question at hand.

More of concern is the weakening of a clean event study approach once further lags are considered. Iterative spillovers (i.e. reflection) between other nearby firms subsequently reducing their employment counts will be present. These are likely to become more pronounced overtime. The total annual changes estimated will therefore be a combination of the direct impacts from the masslayoff event and the indirect, iterative spillovers from other firms adjusting their employment in response. The method used here is unable to decompose the total effect into the direct and indirect spillovers, and so the final figures should be read as a combination of the direct and indirect effects.

A back of the envelope calculation is possible to calculate the cumulative change, aggregating the initial impact and subsequent lagged impacts. The change in log employment can be approximated with percentages, and multiplied out over the five lags. Figure 2.6 displays the results this approximation. The black line with circular points is the cumulative impact at the firm level after five years. The dotted grey lines are the earlier cumulative impacts starting from the initial impact (lowest line) and building up through the lags.

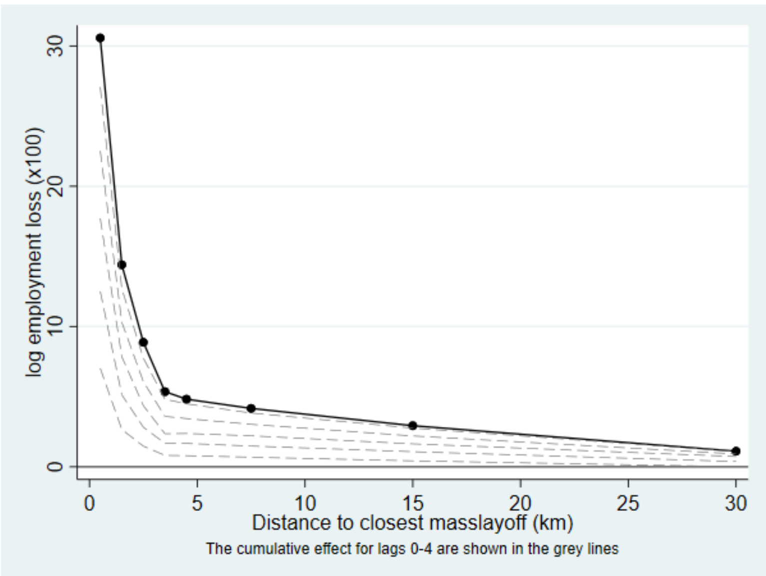


Figure 2.6: Cumulative firm level employment loss overtime, calculated from dynamic estimates

As is seen, the large annual impacts within one and two kilometers reinforce each other substantially overtime. The cumulative impacts are very large in close proximity to the event and decay very rapidly. The calculations are rough approximations and should be viewed in a critical light. I am multiplying out firm level impacts calculated for firms alive at each subsequent time period. As the stock of firms exhibits churn year to year, I am not using the same set of firms for each annual calculation. Therefore the results are not necessarily representative of a firm that is present for the full five lags. Measurement error and the aforementioned issues with reflection are also likely to be magnified with such a calculation. Nonetheless Figure 2.6 provides an interesting visual approximation of the amplified firm level effects overtime and their rapid decay rate.

2.6 Potential spillover channels

I now turn to considering some possible channels through which spillovers operate. The sources investigated here are industrial closeness, labour market closeness, industry input-output linkages and local demand spillovers.

2.6.1 Potential channel: Industrial closeness

I asses the degree to which industrial similarity matters for masslayoff spillovers. If a nearby non-masslayoff firm shares the same industry as the closest masslayoff, we might expect firm level employment spillovers to be stronger. This would occur if knowledge sharing agglomeration was an important driver behind spillovers, and if knowledge sharing is strongest between firms in similar industries.

To unpick this phenomenon, for each firm in the dataset I construct a set of dummies equal to one if the firm's closest masslayoff shares the same industry SIC code to the one, two, three or four digit level. These are included in turn as both level controls and interaction terms with a distance function. Using the full set of non-parametric dummies and four different levels of industry gradation would be difficult to interpret. To more succinctly capture the issue, I use an exponential decay function (the exponential of the negative euclidean distance to the closest masslayoff). This does place an imperfect parametric form on the distance relationship, but nonetheless shows whether those firms close to the mass layoff ($\exp(-dist) \approx 1$) have different spillovers to those further away ($\exp(-dist) \approx 0$). The results are displayed in Table 2.4.

As can be seen, the point estimates for both the level and interaction are negative but very rarely significant. It appears that whether or not firms are

Table 2.4: Industry of closest masslayoff

	(1) $\Delta log(L)$	(2) $\Delta log(L)$	(3) $\Delta log(L)$	(4) $\Delta log(L)$
exp(-dist)	-0.114***	-0.114***	-0.112***	-0.110***
	(0.0110)	(0.0109)	(0.0108)	(0.0109)
1D match	-0.00579**			
	(0.00176)			
exp(-dist) * 1D match	-0.00885			
	(0.0174)			
2D match		-0.00802		
		(0.00454)		
exp(-dist) * 2D match		-0.0129		
		(0.0320)		
3D match			-0.0220	
			(0.0117)	
exp(-dist) * 3D match			-0.0885	
			(0.0676)	
4D match				-0.0402*
				(0.0194)
exp(-dist) * 4D match				-0.201**
				(0.0750)
Cons	-0.0440***	-0.0439***	-0.0440***	-0.0439***
	(0.00652)	(0.00656)	(0.00661)	(0.00665)
Controls	Yes	Yes	Yes	Yes
N	66,014,522	66,014,522	66,014,522	66,014,522
R^2	0.023	0.023	0.023	0.023

Standard errors in parentheses, clustered on 2 digit industry. Exp(-dist) is the exponential of the negative distance to the closest masslayoff. XD match is a dummy equal to one if plant SIC matches closest masslayoff SIC to the X digit level. Fixed effects are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

industrially related to the closest masslayoff is not a key determinant of spillovers. Proximity dominates regardless of industrial closeness.

Only once the firm in question and its closest masslayoff share a four digit SIC code - extremely closely related - do we see any significant and meaningful impact. Extremely closely related firms that are also located very close to the mass layoff appear to suffer additional employment loss. However, this is a very small and specific subset of firms and so the result should not be interpreted as substantial. The main message from the analysis appears to be that industrial closeness is not a key determinant of the degree of spatial spillovers.

2.6.2 Potential channel: Labour market closeness

Similarly, one might expect knowledge spillovers to operate through the labour market. Firms in industries that share similar labour forces as the masslayoff may be more exposed to knowledge agglomeration externalities and therefore more negatively affected by masslayoffs.

To measure the degree to which a firm shares labour markets with its closest masslayoffs, I use the UK Annual Survey of Hours and Earnings (ASHE). The ASHE provides employer reported wage information for a 1% sample of UK workers, selected based on National Insurance Numbers (tax identification numbers). Using the worker panel dynamics of the ASHE, I construct a matrix of employment flows between 2-digit industry codes (SIC07 codes). If a firm of industry A is located closest to a masslayoff of industry B, I construct a measure of labour sharing as the maximum of:

- The percentage of industry A's employment that flows to industry B (labour outflows) and;
- The percentage of industry A's employment that originates from industry B (labour inflows)

The maximum of these percentages is a measure of the firm's (industry A) reliance on the masslayoff's (industry B) labour market. I then merge these maximum flow measures back into the BSD employment-masslayoff panel. The sample of firms is then segmented based on firm percentiles of the labour market closeness measure. Three thresholds are used; the 50th percentile (top 50% of firms), the 75th percentile (top 25% of firms) and the 90th percentile (top 10% of firms) of labour market closeness. Firm's are deemed to share the masslayoff

Table 2.5: Segmenting sample on the 'high' degree of labour market closeness

	(1)	(2)	(3)	(4)
	$\Delta log(L)$	$\Delta log(L)$	$\Delta log(L)$	$\Delta log(L)$
Sample:	50% most related	25% most related	10% most related	Remaining 90 %
dist 0-1km	-0.0740***	-0.0777***	-0.0714***	-0.0685***
	(0.0105)	(0.0131)	(0.0159)	(0.00650)
dist 1-2km	-0.0282***	-0.0272***	-0.0212*	-0.0265***
	(0.00585)	(0.00600)	(0.00865)	(0.00335)
dist 2-3km	-0.0148**	-0.0133*	-0.00783	-0.0148***
	(0.00545)	(0.00601)	(0.00879)	(0.00275)
dist 3-4km	-0.00794	-0.00637	-0.000412	-0.00825**
	(0.00493)	(0.00557)	(0.0100)	(0.00264)
dist 4-5km	-0.00738	-0.00523	-0.000967	-0.00789**
	(0.00465)	(0.00457)	(0.00748)	(0.00257)
dist 5-10km	-0.00651	-0.00489	0.000670	-0.00705***
	(0.00415)	(0.00417)	(0.00674)	(0.00179)
dist 10-20km	-0.00297	-0.00158	0.00146	-0.00414***
	(0.00376)	(0.00336)	(0.00614)	(0.00118)
dist 20-40km	-0.000580	-0.000261	0.00151	0.000445
	(0.00324)	(0.00257)	(0.00542)	(0.00166)
Cons	-0.0380***	-0.0447***	-0.0470**	-0.0434***
	(0.00597)	(0.0128)	(0.0158)	(0.00653)
Controls	Yes	Yes	Yes	Yes
N	31,883,890	16,041,175	6,286,332	59,694,725
R^2	0.022	0.025	0.028	0.023

Standard errors in parentheses, clustered on 2 digit industry. Variables are a set of dummies indicating distance to closest masslayoff. Controls are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

market to a 'high degree' if they are above the relevant percentile cutoff, and to a 'lower degree' if below.

The results are displayed in table 2.5. The distance estimates of spillover effects do not vary significantly with the sample segmentation. Firms sharing labour markets with their closest mass layoff are not more strongly affected by spillovers than other firms. Labour market closeness, and the theorised knowledge spillovers, do not appear to be operating strongly here.

2.6.3 Potential channel: Input-output related industries

Another commonly cited source of local agglomeration and employment spillovers is vertical linkages between firms. Perhaps the employment spillovers found in this analysis are down to input-output linkages between mass layoffs and very local firms. Already the degree of localisation suggests this may not be the case as it is unlikely that primary suppliers or buyers are located within a couple of kilometers.

I repeat a similar analysis based on the degree of input-output linkages between a firm's industry (industry A) and the industry of their closest masslayoff (industry B). To do use, I use the ONS input-output tables available at the 2 digit industry level. From the 2006 industry I-O tables, I calculate the following percentages:

- Upstream degree: the percentage of an industry A's output that is sold to industry B. This is flow from A to B divided by the total output of A (final demand of A).
- Downstream degree: the percentage of industry's A's intermediate inputs that are sourced from industry B. This is the flow from B to A divided by the total intermediate purchases by industry A.

These percentages are then merged back into the BSD firm panel using the 2 digit industry codes of the firm (industry A) and their closest masslayoff (industry B). Similar to the labour market closeness analysis, I then use firm percentile cutoffs to segment firms into those that are 'highly upstream', 'highly downstream' or 'neither highly upstream nor highly downstream' to their closest masslayoff.

Table 2.6 displays the non-parametric estimates of employment spillovers for different input-output linked firms. Column one estimates the spillovers of the 25% of firms in industries with the highest upstream linkages to their closest masslayoff's industry. (The firm's industry sells a large proportion of their output to the masslayoff's industry). Column two estimates the spillovers for the

Table 2.6: Segmenting the sample based on degree of upstream and downstream relationship to closest masslayoff

	(1)	(2)	(3)	(4)
	$\Delta log(L)$	$\Delta log(L)$	$\Delta log(L)$	$\Delta log(L)$
Sample	25% most upstream	25% most downstream	Neither upstream nor downstream	All firms
dist 0-1km	-0.0613***	-0.0737***	-0.0700***	-0.0702***
	(0.00854)	(0.0150)	(0.00678)	(0.00769)
dist 1-2km	-0.0231***	-0.0240**	-0.0262***	-0.0266***
	(0.00573)	(0.00713)	(0.00345)	(0.00362)
dist 2-3km	-0.00748	-0.0109	-0.0174***	-0.0147***
	(0.00510)	(0.00644)	(0.00309)	(0.00305)
dist 3-4km	-0.000153	-0.00484	-0.0107***	-0.00813**
	(0.00449)	(0.00635)	(0.00287)	(0.00269)
dist 4-5km	-0.000973	-0.00500	-0.00892**	-0.00792**
	(0.00455)	(0.00559)	(0.00297)	(0.00254)
dist 5-10km	-0.000749	-0.00369	-0.00802***	-0.00683***
	(0.00401)	(0.00478)	(0.00207)	(0.00179)
dist 10-20km	0.000886	-0.00225	-0.00413**	-0.00424***
	(0.00393)	(0.00415)	(0.00125)	(0.00119)
dist 20-40km	0.00371	-0.000457	0.000655	-0.0000507
	(0.00240)	(0.00372)	(0.00189)	(0.00121)
Cons	-0.0328***	-0.0198**	-0.0468***	-0.0426***
	(0.00593)	(0.00578)	(0.00633)	(0.00641)
Controls	Yes	Yes	Yes	Yes
N	14,838,080	14,451,151	38,779,548	66,014,522
R^2	0.020	0.022	0.024	0.023

Standard errors in parentheses, clustered on 2 digit industry. Variables are a set of dummies indicating distance to closest masslayoff. Controls are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

25% of firms in industries with the highest downstream linkages to their closest masslayoff's industry. (The firm's industry sources a large proportion of their inputs from the masslayoff's industry). For comparison, column three presents the estimates for firms that are neither in the 25% upstream or 25% downstream samples, and column four presents the pooled estimates across all firms.

Again, the results find no statistically different spillover measures between these subsets of firms. The patterns are repeated when we restrict the cutoffs further by comparing the top 10% of upstream and downstream firms with the remainder. It would appear that industry input-output linkages are not driving the strong, localised employment spillovers.

2.6.4 Potential channel: Local product demand spillovers

Finally, I look at whether or not firms in non-tradeable industries experience stronger employment spillovers from masslayoffs. Non-tradeable goods are the most responsive to local consumer spending. In turn, local consumer spending is likely to be hit following a masslayoff as the newly laid off workers reduce their consumption. Therefore, if firms in non-tradeable industries experience more negative spillovers from masslayoffs, this would point towards local spending as a transmission channel.

I calculate the degree of tradeability for industry A using the same two digit input-output tables from the ONS. In short, the degree of tradeability measure captures the international integration of each industry. The index measure is the maximum of:

- Export fraction: the fraction of industry A's total output that is exported (rather than sold/consumed domestically).
- Import fraction: the fraction of industry A's total intermediate inputs that is sourced from imports (rather than domestic sources).

The measure is then merged back into the BSD firm panel using the firms' 2 digit industry codes. I segment the set of firms in the same way according to percentile fractions. Firms above percentile cutoffs are considered in 'tradeable' industries, and firms below are considered in 'non-tradeable' industries.

Results are displayed in Table 2.7. As we see, the 10% of firms in the least tradeable industries experience the strongest spillovers, particularly at the closest distances to the mass layoff. Non-tradeable firms still exhibit employment spillovers but to a lesser extent. Local demand channels are therefore likely to be one channel contributing to the observed spillovers.

Table 2.7: Highly non-tradeable vs not highly non-tradeable industries

	(1)	(2)	(3)	(4)
	$\Delta log(L)$	$\Delta log(L)$	$\Delta log(L)$	$\Delta log(L)$
Sample	50% least tradeable	25% least tradeable	10% least tradeable	Remaining firms
dist 0-1km	-0.0704***	-0.0701***	-0.116***	-0.0668***
	(0.0118)	(0.0165)	(0.00307)	(0.00721)
dist 1-2km	-0.0227***	-0.0201*	-0.0353***	-0.0255***
	(0.00529)	(0.00816)	(0.00208)	(0.00390)
dist 2-3km	-0.00982**	-0.00836	-0.0157*	-0.0147***
	(0.00353)	(0.00393)	(0.000811)	(0.00353)
dist 3-4km	-0.00515*	-0.00490*	-0.00717	-0.00833**
	(0.00236)	(0.00169)	(0.00391)	(0.00320)
dist 4-5km	-0.00500**	-0.00466***	-0.00511	-0.00825**
	(0.00170)	(0.000963)	(0.000603)	(0.00306)
dist 5-10km	-0.00312*	-0.00227*	-0.00217	-0.00747***
	(0.00129)	(0.000894)	(0.000548)	(0.00212)
dist 10-20km	-0.00330***	-0.00289***	-0.00351	-0.00438***
	(0.000699)	(0.000564)	(0.000660)	(0.00146)
dist 20-40km	-0.00129*	-0.00123	0.00142	-0.0000346
	(0.000468)	(0.000658)	(0.000855)	(0.00138)
Cons	-0.0424***	-0.201***	-0.181***	-0.0442***
	(0.00669)	(0.00941)	(0.00486)	(0.00698)
Controls	Yes	Yes	Yes	Yes
N	33,290,051	18,359,195	7,481,532	57,748,254
R^2	0.022	0.020	0.019	0.023

Standard errors in parentheses, clustered on 2 digit industry. Variables are a set of dummies indicating distance to closest masslayoff. Controls are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.7 Conclusion

The paper has provided the first estimates of employment spillover decay rates over space and time of masslayoffs. Spillovers on nearby firms are strongly negative but very highly localised. They are also very persistent, despite the one-off nature of the masslayoff shock. Individual firms in close proximity continue to reduce their employment year-on-year for at least five years after the event.

The degree of localisation matters both for the measurement of the spillovers and for policy targeting. As to the measurement issue, Section 2.4 explicitly addresses the costs of measuring spillovers over large discrete units. With the spillovers as localised as they are, averaging over large administrative or commuting zone units, as is the common approach in spatial labour market analysis, will tend towards estimating no spillover effects. This is clearly not the case as the zero results stem from looking at a misleading area.

The localisation and their dynamics are important for policy responses too. We do observe a large initial shock generating persistently negative local spillovers. If a policy maker wishes to mitigate the negative 'snowball effect' the results would imply that any intervention, such as support to nearby firms or workers at the outset of the shock, should be very local in nature. Intervention over a broader area would be more costly and poorly targeted. Beyond this observation, I leave the evaluation of any particular policy intervention's effectiveness to other discussions.

Section 2.6 considers possible local firm linkages that could be contributing to the local spillovers. No support is found for input-output spillovers, shared labour market spillovers (which would indicate possible knowledge agglomeration spillovers), or within-industry productivity spillovers. It should be noted that the first two were only able to be measured at the two-digit industry level for data availability reasons. The lack of findings must therefore be interpreted in this light.

The results do however find some evidence of local product demand spillovers as non-tradeable firms reduce their employment more strongly than their tradeable counterparts. These local demand effects are found around the masslayoff, therefore around the place of work of laid off workers. However, households are likely to spend a large fraction of their income around their place of residence. An interesting corollary of the local demand results would be to investigate whether non-tradeable firms in residential areas are affected by local residents experiencing a mass layoff. The data available do include place of residence from 2004 so such an extension can be feasibly implemented.

Chapter 3

Hours of work polarisation?

Abstract

We investigate the relationship between hours per worker and employment polarisation. Our core question is whether hours per worker follow the same polarisation patterns as previously observed for employment, measured by either heads or total hours. Using the occupational task index measures of Acemoglu and Autor (2011), we find large relative declines in hours per worker in routine manual jobs – precisely the occupations most negatively affected by employment polarisation from routine-biased technical change. We also find a lower relative decline in hours per worker for non-routine cognitive analytical jobs, which are growing through polarisation. At the same time, hours per worker declined significantly more than the trend for non-routine manual physical occupations. Instead of a polarisation pattern, we find that hours per worker have been declining more in manual jobs (routine manual and non-routine manual physical). These patterns are observed across age, gender and education groups, with few exceptions and changes in intensity. The decline in hours per worker occurred mostly within sectors. Using a wage ranking of occupations instead of occupational task indices, the decline in hours per worker is monotonically related to wages. The results are specific to the European countries and the same patterns are not found using data for the United States.

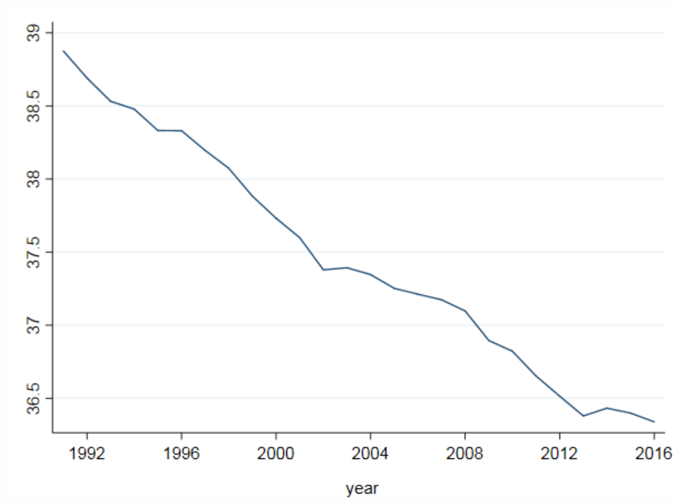
Keywords: job polarisation, hours per worker, routine-biased technical change

JEL codes: J23, J24, O33

3.1 Introduction

Employment polarisation has emerged as a widespread phenomenon across advanced economies in the past three decades. It is characterised by a decline in the share of middle-skill jobs and an increase in the share of high- and low-skill jobs (conventionally defined by a one-on-one mapping of wages to skills), leading to the well-known "hollowing-out" of employment. Polarisation has occurred alongside a longer-term decline in the intensive margin of employment (hours per worker) in European economies. Figure 3.1 shows the evolution of usual weekly average hours of work per worker across EU-15 countries from 1992-2016, which have fallen by almost two and a half hours during that period, generating substantial aggregate effects on total hours in the economy. The fall is not isolated or confined to small groups of countries. Instead, it forms part of a longer term trend of declining hours worked per employed person across European economies. The two labour market trends, while not necessarily sharing causal factors, are likely to have interconnected implications and yet have only been analysed in isolation. We investigate whether hours per worker exhibit polarisation patterns along similar distributional lines as already documented employment polarisation.

Figure 3.1: Average weekly hours per worker in EU-15 countries, 1992-2016



Source: EU Labour Force Survey, authors' own calculations. Hours refers to usual weekly hours per worker.

The non-monotonic relationship between skills and employment that characterises job polarisation first emerged in the 1990s. For the United States, Acemoglu and Autor (2011) show that the process of employment polarisation occurred largely over two periods. An initial period between 1989 and 1999 was characterised by a strong increase in the share of high-skilled jobs, a smaller increase in the share of low-skilled jobs and a decline in middle-skilled jobs. A second period followed, between 1999 and 2007, where the share of employment grew most in the last third of the wage distribution. With a view to explaining the new employment composition patterns, Autor et al. (2003) developed the hypothesis of routine-biased technical change (RBTC). Traditionally middle-skilled jobs requiring repetitive routine tasks – such as factory production lines or clerical work – are vulnerable to increasing automation and have seen their wages and employment shares decline. Low-skilled jobs, particularly those requiring in-person services – such as cleaning and personal care – are less substitutable for technology and as a consequence their relative employment share has increased slightly. At the other end, high-skilled workers – such as managers and analysts - find their labour complemented by technological progress, increasing their share of employment.

Since the seminal work of Autor et al. (2003), a large body of literature emerged around the theme of employment polarisation. Autor et al. (2006) related the observed employment polarisation in the United States with the changing distribution of in-job task demands related to technological advancement and outsourcing. Goos and Manning (2007) showed evidence of job and wage polarisation in the United Kingdom, while Goos et al. (2009) and Goos et al. (2014) broadened their scope and showed evidence of pervasive job polarisation in 16 European countries. Evidence of job polarisation is also available for other large advanced economies (e.g Coelli and Borland, 2015, for Australia; Green and Sand, 2015, for Canada; Furukawa and Toyoda, 2018, for Japan).

A complementary branch of the literature has focused on specific drivers of employment polarisation. This includes, for example, the role of low-service occupations in the rise of polarisation (Autor and Dorn, 2013), the contribution of ICT to the polarisation of the labour market via an increase in demand for high-skilled labour (Michaels et al., 2014), the types of workers who transit from routine to non-routine cognitive and non-routine manual jobs (Cortes, 2016), the role of specific demographic groups in shaping employment polarisation (Cortes et al., 2017) and the relationship between polarisation patterns and inter-industry wage differentials (Shim and Yang, 2018). More recently, vom Lehn (2019) tests the hypothesis of substitutability and complementarity of different workers with machines and found that patterns of polarisation do not always conform to that hypothesis over time. He hypothesises that more recent technological change could have evolved to replace also analytical tasks.

A key observation is that existing work on employment polarisation focuses on either headcount or total hours worked. To our knowledge, the internal margin of adjustment – hours per worker – has not been analysed, and that is what we study in this paper. Contrary to employment, which has been increasing during our sample, hours per worker are on a long-term declining trend. Employment polarisation and the decline in hours per worker are far apart in time but they may both have, as many other changes in labour markets, a common underlying pattern: technological progress.

Taking the manufacturing sector alone, in the period 1913-1997 hours worked annually per person declined by 31% in the United States, 38% in the United Kingdom, 42% in France and 45% in Germany (Cahuc et al., 2014). The trend has not been uniform over time and across countries. Periods of sharp decline in average hours worked have been followed by periods of stabilisation or even an increase in average hours worked. Average hours worked per person also tend to be substantially higher in low- than in high-income countries (Bick et al. (2018)). Various factors could be at play in the determination of hours worked per person such as, for example, labour laws, unionisation, taxation and home sector productivity. However, Vandenbroucke (2009) argues that these forces appear of secondary importance relative to technology.

The evolution in hours worked also appears to be heterogeneous across skill groups. Since the 1980s, the average hours of low-skilled workers have dropped significantly whereas those of high-skilled workers have remained high (Aguiar and Hurst, 2007 and Boppart and Ngai, 2018). The timing of change in hours worked by educational group coincides with the increase in inequality in wages and consumption (Katz and Autor, 1999); it is also in the neighbourhood of the early stages of the process of job polarisation (Autor et al., 2003).

Our framework models a statistical rather than causal relationship between hours per worker and job polarisation. We ask whether hours per worker follow the same distributional patterns as employment, and less formally investigate possible mechanisms. We also try to infer the possible income distribution consequences of the observed patterns in hours per worker. There are good reasons why polarisation in employment could also affect hours per worker. First, hours per worker could be affected by the same demand forces affecting employment polarisation. Second, technological advancements may change the degree of substitutability between capital and labour. For instance, technological advances have allowed for better monitoring of consumer demand and scheduling of labour. This might be particularly relevant in services sectors.

The value of uncovering a relationship between hours of work and job polarisation is twofold. In a purely mechanical sense, a pattern of hours per worker polarisation would exacerbate already documented employment and wage polarisation. Arguably, it is overall income polarisation – rather than hourly wage or headcount employment - that is of primary concern to policy makers concerned with distributional issues. Total income is the multiplicative function of employment (extensive margin), wages and hours of work (the intensive margin). The third contributing variable – hours – has been absent from the polarisation literature. If hours polarise along similar lines as wages and employment, that would have an exacerbating effect of polarisation. Higher employment with lower hours worked per person in lower-paying occupations together with higher employment with relatively more hours worked per person in high-paying occupations could increase concerns about the quality of jobs at the bottom of the wage distribution and would exacerbate wage inequality.

The second motivating factor for studying patterns in hours per worker is to gain a better understanding of how the relatively new phenomenon of job polarisation is related to the long-term trend decline in hours worked per person. A fundamental difference between the analysis of job polarisation based on employment or total hours and average hours is that while employment and total hours have been increasing, average hours have been declining. An empirical question we attempt to answer is whether hours per worker are declining across all skill levels or they exhibit patterns of polarisation similar to those observed for employment and wages.

Our empirical analysis uses the EU-LFS data for the period 1992-2016 for the EU-15 countries.[1] We follow the recommendation of Autor (2013) that researchers use, as far as possible, available measures of tasks classification, and utilise available indices of job task and skill content – particularly routinisation – to explain trends in hours per worker. We use six indices to classify job tasks instead of the common three categories. Non-routine cognitive jobs are divided between analytical and interpersonal; routine jobs are divided between cognitive and manual and non-routine manual jobs are divided between physical and (inter)personal. In particular, we match occupation indices from Acemoglu and Autor (2011) to EU-LFS microdata from 1992 to 2016, focusing on EU-15 countries. In addition, we also use their index of offshorability to account for the potential of outsourcing. The task indices are shown to explain employment and wage polarisation in both

[1] The EU-15 countries comprise Austria, Belgium, Denmark, France, Finland, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden and the United Kingdom. Data exist for all 15 countries for the period 1992-2016, with the exception of Austria, Finland and Sweden where data start in 1995. Further, occupation identifiers are available for Belgium from 1993 and for Finland and Sweden from 1997, and so these countries enter our sample then.

the US and the EU. We investigate whether they additionally explain the level and, more importantly, trend in hours per worker.[2]

We find that workers in highly routine manual jobs are working fewer hours relative to other occupations over time – the interaction term between a high degree of routinisation and a time trend is negative and significant. However, this result is only observed for routine manual jobs and is broadly absent for routine cognitive jobs. We also find that workers are working relatively more hours in non-routine cognitive analytical jobs, although the evidence there is somewhat weaker. Taken together, these two patterns give partial support to the hypothesis that hours per worker follow the same polarisation patterns as employment at the top. For non-routine manual jobs the evidence is more mixed. While we observe a relative increase in hours per worker in non-routine manual interpersonal jobs, we see the opposite for non-routine manual physical jobs. The two effects combined make hours per worker for non-routine manual jobs relatively neutral as compared to the trend in hours. However, for non-routine manual jobs the occupational task indices also do not give the typical polarisation patterns for total employment.

Overall, instead of a polarisation pattern, our results show that hours per work declined more in manual jobs (routine manual and non-routine manual physical jobs). Our results remain robust to the partition of the sample between full-time and part-time, between male and female and across education and age groups. Hours worked per person also declined more than the trend in jobs that are highly offshorable. The main patterns are also consistent across the EU-15 countries with few exceptions.

Additionally, we analyse patterns in hours per worker along the wage distribution, instead of the occupational indices. While for total employment we see the typical U-shaped polarisation pattern, for hours per worker the pattern is inverted L-shaped. Put differently, the share of employment declined in middle-wage occupations in relation to bottom and top occupations, whereas hours per worker show a monotonic relationship with wages: hours per worker declined more in low-wage occupations, followed by middle-wage occupations and high-wage occupations have experienced almost no decline in hours per worker. If we partition the wage distribution in six quantiles instead of three, we observe a U-shaped pattern for most of the distribution, but with a lower decrease in hours per worker at the top

[2]As is standard, we assume skills are increasing in wages. For convenience, we also assume the conventional mapping carries over to tasks, whereby occupations intensive in non-routine cognitive tasks are high-skilled, occupations intensive in routine tasks are middle-skilled, and those intensive in non-routine manual tasks are low-skilled. Throughout the text, we retain this implicit mapping and refer to individuals in jobs with a high content of non-routine cognitive as high-skilled, and similarly for other tasks.

quantile. Thus, contrary to occupational indices where hours declined more in the middle, the wage ranking of occupations shows that hours declined more at the bottom.

Taken together, our results suggest that patterns in hours per worker exacerbate the impact of polarisation on wage inequality. High-skilled workers increased their share of employment and work relatively more hours; medium-skilled workers saw a decline in the share of employment and a decline in hours per worker; and low-skilled workers saw a substantial decrease in hours per workers and a smaller increase in their share of employment. The analysis based on the wage ranking of occupations makes this point even clearer: hours per worker declined significantly more in low-paying occupations.

The contributions of this paper are threefold. First and foremost, we document a new stylised fact: trends in hours per worker vary substantially across occupational task indices – taking together the two constituents of each index, at the top (non-routine cognitive) and the middle (routine), hours per worker evolved similarly to employment polarisation patterns, while at the bottom (non-routine manual) they declined. If we instead use a wage rank of occupations, hours per worker decline monotonically with wages. The difference between the two approaches is that the decline in the middle-skill jobs happens via a decline in routine manual jobs which are in fact low paid. This also shows the importance of a finer disaggregation of skills suggested by Acemoglu and Autor (2011), which we employ in this paper. The direct effect of these mechanisms has been to exacerbate overall earnings polarisation. Second, we contribute to the empirical literature relating the long-term decline in hours worked with technological advancement. Third, we provide detailed level country results that show broadly similar patterns across EU-15 countries, and across a variety of demographic groups.

The rest of the paper is structured as follows. Section 3.2 describes the European Union Labour Force Survey (EU-LFS) data, the construction of the occupational task indices used and shows main descriptive statistics. Section 3.3 presents baseline results for hours per worker across task indices while Section 3.4 considers additional contributing factors. The latter includes demographic shifts over the past few decades, offshorability of jobs, part-time employment and industrial change. Section 3.5 decomposes overall employment polarisation into the hours-per-worker channel and head-count employment channel. Section 3.6 turns to country-level analysis – the individual EU-15 countries and a comparison with the United States. Section 3.7 concludes.

3.2 Data and descriptive statistics

The main dataset used in this paper is the EU Labour Force Survey (EU-LFS). The EU-LFS is a microdataset of repeated cross sections of household level observations (in our case, of annual frequency). As the EU-LFS data are collected based on a harmonised methodology they are suitable for cross-country analyses. Additionally, we use the EU Survey on Income and Living Conditions (EU-SILC) in order to obtain information on wages, as wage variables are missing in our version of the EU-LFS dataset.

Two key variables in our analysis are hours worked per person and individuals' occupations. Hours worked are reported in reference to the week prior to the survey. Individuals are asked about the hours usually worked and the hours actually worked. Differences between the two measures include, among others, overtime or downtime, holidays and sick leave. We focus on working individuals reporting positive usual hours worked per week in the EU-15 countries, during the period 1992-2016, and will only refer to usual hours for the remainder of the paper, unless otherwise stated.

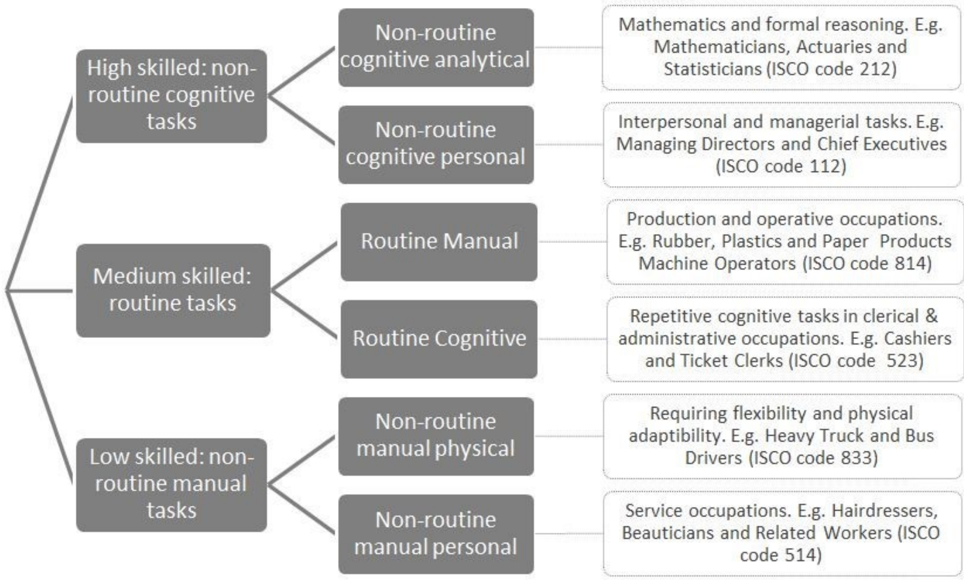
Occupations are codified according to the International Standard Classification of Occupations (ISCO). Each individual worker's occupation is classified with 3-digit ISCO occupation codes: ISCO88 codes for observations up until 2010, and ISCO08 codes for observations from 2011 onwards.[3] The EU-LFS also provides detailed demographic information such as age, gender, education attainment, family status, etc. It also provides other employment information, such as size of firm and, also important for our analysis, the worker's sector of economic activity, according to the Statistical classification of economic activities in the European Community (NACE).

Our analysis centres on measures of occupational skill and task content. We base our analysis on the job skill measures created by Acemoglu and Autor (2011). They use O*NET data on work abilities, work activities, work context and skills to have composite measures to classify each occupation according to their propensity for use of five tasks, and a measure of offshorability. The skill characteristics form three broad groupings - non-routine cognitive, routine, non-routine manual – approximating the top, middle and lower ends of the labour market respectively.

Much of the polarisation literature explicitly or implicitly finds that three skill categories – high, medium and low - are insufficient to capture structural changes in the labour market, as technological impacts are heterogeneous within each skill

[3] Wage analysis is based on 2-digit ISCO occupation codes.

Figure 3.2: Mapping of skills, tasks and occupations



grouping. For example, Autor and Dorn (2013) find that the growth in lower skill employment is driven by in-person, service sector jobs. In the middle segment of the labour market, routine manual labour on factory production lines was largely automated in an initial round of RBTC. Arguably, a second round of RBTC, in the form of computerisation, is in the process of replacing routine cognitive jobs, such as cashiers and law clerks. In an attempt to capture richer technological impacts on heterogeneous tasks, we opt for the finer gradation of Acemoglu and Autor (2011). We additionally create a sixth measure that separates lower skilled, non-routine manual jobs into personal services and physical jobs.[4] Figure 3.2 displays the six task indices, paired by skill level, their corresponding tasks and an example occupation that requires high levels of the relevant index. Throughout the text, we refer to the six task indices as occupational task indices.

The occupations used in Acemoglu and Autor (2011) are the SOC2000 measures and each index is distributed approximately standard normal. We use a number of crosswalks to match the indices to the 3-digit ISCO codes used in the EU-LFS.[5]

[4]The category of non-routine manual personal is not part of Acemoglu and Autor's 2011 handbook chapter, but it is available in their online data programmes. We have complemented their measures with other O*NET context and ability task measures (Appendix C.1). We impose a further restriction and remove from the index all occupations with codes below 300 (managers and professionals). This was done because doctors, veterinarians, midwifery professionals and other similar professionals were ranking high in the newly constructed non-routine manual personal index, which made it difficult to distinguish from non-routine analytical personal jobs. The results we report in this paper relate to our modified measure. Results for the original measure by Acemoglu and Autor can be found in Appendix C.4

[5]We use a crosswalk from SOC2000 to ISCO88 to transform the indices to ISCO88 codes. Where multiple SOC2000 occupations match ISCO88, we take the weighted mean of each index

The final result is a table of ISCO occupation codes and the corresponding values for each of the six indices, for each occupation. A note on terminology: all jobs are characterised by a combination of several different skills and tasks. For brevity, we will refer to jobs characterised by a high content of, e.g., routine cognitive tasks as routine cognitive jobs.

Figure 3.3: Occupational task indices and personal characteristics

Source: EU-LFS and authors' calculations.

Figure 3.3 shows some characteristics of our sample. Weekly hours per worker declined by 2.4 hours between 1992 and 2016, primarily in routine manual and non-routine manual jobs. By contrast, hours per worker declined only marginally

using the US 2000 Census weights provided by Acemoglu and Autor (2011). To match the indices to ISCO08 codes, we again use a crosswalk from ISCO88 to ISCO08 and take the mean of each index for multiple matches.

in routine cognitive and non-routine cognitive analytical jobs. These patterns show the importance of differentiating occupational task indices by more than three categories. The share of part-time work increased across all occupational task indices but more so in routine manual jobs, suggesting that the increase in part-time may have played some role in the accentuated decline in hours per worker in these occupations.[6] The share of part-time is higher among non-routine manual personal tasks, which is also the occupational task index with the largest share of female workers. At the same time, there is no fixed threshold of work-time status; individuals who work sufficiently fewer hours may self-classify as working part-time. In that sense, lower average hours almost mechanically imply more part-time work.

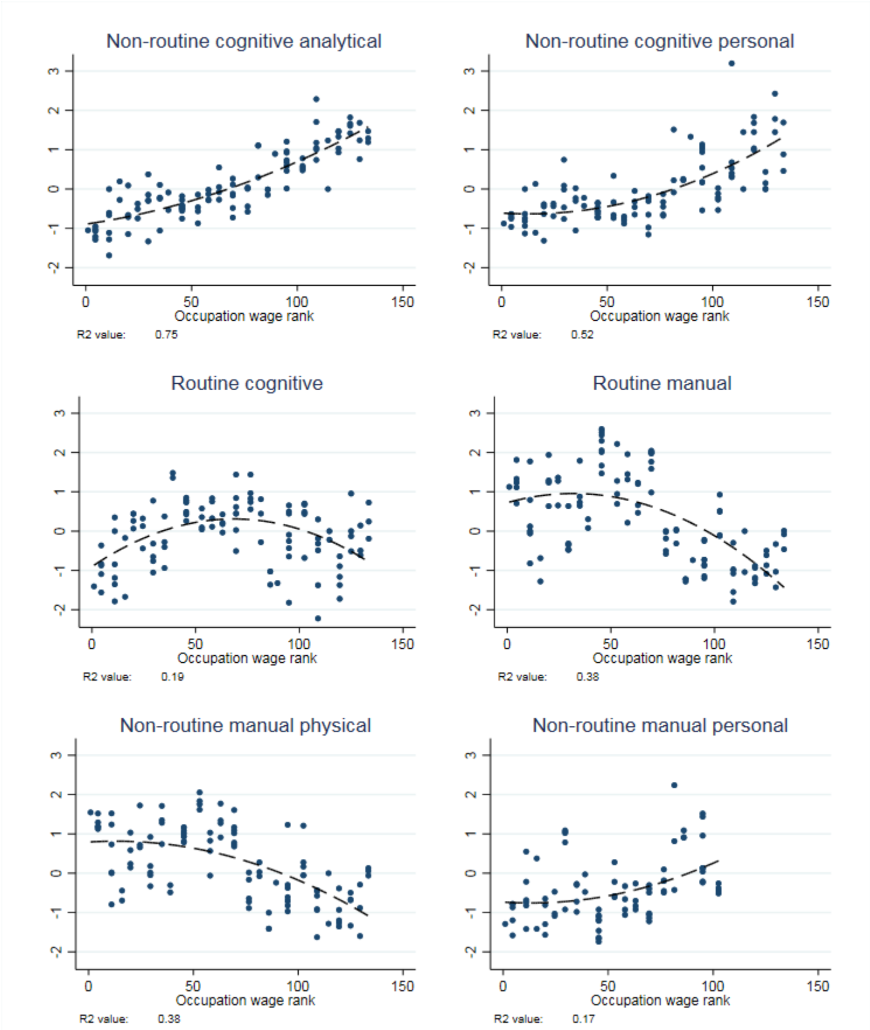
Figure 3.3 also shows the well-known pattern of an ageing workforce and, more importantly for our analysis, that average age increased more in routine jobs. The education shares across the six indices provide additional information of the skills required. Non-routine cognitive tasks are those employing the largest share of highly educated workers. At the bottom, with the lowest shares of highly educated workers, are the non-routine manual physical and routine manual occupations, which are those that display the largest reduction in hours per worker. Routine cognitive jobs stay at the middle of the skill distribution.

Our wage rankings come from individual data from EU-SILC. We construct an hourly wage variable by dividing gross annual employee cash earnings by annual total hours, for both full-time and part-time work, excluding self-employed individuals and family workers, as well as individuals with zero earnings. We pool data across the EU-15 by normalising individual wages by the weighted country average. We then calculate average hourly wages for each of the two-digit ISCO88 occupations in 2008. We use a weighted average using EU-SILC weights, but the unweighted measure is very similar. Other years give similar results, but 2008 gives the largest coverage across countries for the ISCO88 classification. We then link the task indices at the 3-digit ISCO88 with the wage ranking at 2-digit ISCO88, ending up with around 130 occupations for each index, except for non-routine manual personal, which we restrict to not include managerial or professional occupations (100 and 200 level).

Figure 3.4 displays the relationship between occupational task indices and the occupational wage rank, fitted with a quadratic curve. There is a positive relationship between wages and the level of non-routine tasks. By contrast, routine

[6] The share of involuntary part-time is higher among part-time workers in routine manual and non-routine manual physical jobs, which is suggestive evidence that part of the decline in hours per worker is demand driven.

Figure 3.4: Occupational task indices versus wage ranking



Source: EU-LFS, EU-SILC, index construction data from Acemoglu and Autor (2011), O*NET task data and authors' calculations. Wage ranks are calculated using weighted and pooled 2008 EU-SILC data for EU-15 countries.

tasks display the expected inverse U-shaped pattern, highlighting their prevalence in the middle segment of the labour market. The non-routine manual physical index is decreasing in wages, as expected, while the non-routine manual personal is slightly increasing, albeit with a low R^2.

Overall, the descriptive patterns show a more subtle categorisation of the occupational task indices than the usual trichotomy of abstract, routine and manual tasks. In fact, routine manual jobs show characteristics more typical of low-skilled jobs: the share of high-education workers is as low as for non-routine manual physical occupations, as is the wage rank. Conversely, non-routine manual personal occupations appear to be more middle- than low-skilled: the share of high education is similar to routine cognitive jobs, while the wage rank cuts across almost the entire distribution. Indeed, "personal" seems more challenging to pin down using the occupational task indices compared to other jobs - though it emerges clearly that "personal" occupations have a large share of female workers.

3.3 Baseline results

In this section we estimate a reduced form model to analyse the relationship between the indices discussed in the previous section – known to explain employment and wage polarisation – and weekly hours of work per worker. We are interested primarily in whether the indices can account for the trend in hours per worker, which is decreasing at the aggregate level.

We run the following empirical model:

$$Y_{ikct} = \alpha_0 + \alpha_1 I_i + \alpha_2 t + \alpha_3 I_i * t + \beta X_{ict} + c_k * c_c + \epsilon_{ikct} \tag{3.1}$$

This specification fits the outcome of interest - hours per worker, Y, for individual i in industry k in country c at time t to an intercept, the index value of the individual's occupation, I_i, a linear time trend t and an interaction between the index in question and the time trend. For ease of interpretation, we convert the continuous index measures into dummy variables that equal one if the occupation has a high index score, above the 66^{th} percentile for occupations in each year, and zero if it has a low score.[7] Coefficient α_1 accounts for level differences between different occupations that occur regardless of any trends, while α_2 controls for the aggregate trend. The main coefficient of interest is α_3, which captures the extent to which hours for occupations in a given index trend in a way that differs from the aggregate. Given the overall aggregate trend, a positive value for α_3 indicates that occupations with high values of the index in question have exhibited a milder decline, and opposite for a negative value.

Additional covariates include country and industry fixed effects, demographics (gender, age, education) and controls such as firm size, whether the interview was conducted with the person in question or by proxy. Country and industry fixed effects account for time-invariant heterogeneity in average hours worked across countries or industries. Two-way country and industry fixed effects assume the

[7] We calculate the 66^{th} percentile of each index, each year using the weighted EU-15 LFS observations for the relevant year. This method ensures that for each index, one third of the observations each year will have the dummy variable equal to one. This method thereby abstracts from classification changes over time. As a robustness check, we also repeat the analysis using a constant 66^{th} percentile i.e. the threshold for a given index is the same across years.

industry effect is identical across countries, while an interacted country-industry fixed effect allows for the possibility of heterogeneity across countries within the same industry. Unless stated otherwise, errors are clustered at the country-industry level, in a total of 240 clusters. All regressions are weighted using EU-LFS weights, as provided by Eurostat.

The results are based on the estimation of specification 3.1 one time per index on the total sample. We start by analysing the explanatory ability of the non-routine cognitive indices for hours per worker. Table 3.1 presents baseline regressions of hours per worker on the time trend, a dummy for the individual working in a high index score occupation, the interaction and covariates as specified above. The first three columns refer to the non-routine cognitive analytical index and the final three to the non-routine cognitive personal index. Columns 1 and 4 present results without controls, columns 2 and 5 with the standard controls, country and sector fixed effects, and columns 3 and 6 with controls and country-sector interacted fixed effects.

Table 3.1: Baseline results for non-routine cognitive (analytical and personal) indices

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	———- Analytical ———-			———- Personal ———-		
High Index*t	0.0500*	0.0873**	0.0410	-0.0089	0.0352	0.0129
	(0.0272)	(0.0366)	(0.0289)	(0.0275)	(0.0317)	(0.0269)
High Index	2.5290***	1.6709*	2.8334***	2.2450***	1.7544**	3.2951***
	(0.8698)	(0.9351)	(0.7622)	(0.8324)	(0.8500)	(0.6076)
t	-0.1182***	-0.0957***	-0.0818***	-0.1006***	-0.0856***	-0.0790***
	(0.0154)	(0.0137)	(0.0135)	(0.0159)	(0.0118)	(0.0117)
Constant	37.9281***	39.6241***	41.7552***	38.0525***	39.1187***	41.1743***
	(0.5777)	(1.2525)	(0.5693)	(0.5342)	(1.2703)	(0.5475)
Observations	21561515	16754427	16754427	21561515	16754427	16754427
R-squared	0.0193	0.1575	0.2082	0.0103	0.1530	0.2086
Controls	No	Yes	Yes	No	Yes	Yes
Country FEs	No	Yes	No	No	Yes	No
County-Sector FEs	No	No	Yes	No	No	Yes

Note: All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index in that year. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016, hours variable is 'usual' hours of work.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

High non-routine cognitive analytical and high non-routine cognitive personal jobs are both associated with higher initial hours per worker. The interaction term with the time trend is positive but only weakly significant for the first two specifications for non-routine cognitive analytical jobs. When the most stringent country-sector interaction fixed effects are included, removing more variation,

Table 3.2: Baseline results for routine (cognitive and manual) indices

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	Cognitive			Manual		
High Index*t	0.0769***	0.0215	0.0187	-0.2083***	-0.1799***	-0.1246***
	(0.0176)	(0.0171)	(0.0131)	(0.0390)	(0.0264)	(0.0192)
High Index	-2.2284***	-0.2970	-0.9371***	4.9924***	2.6383***	0.9427***
	(0.4875)	(0.4780)	(0.2667)	(0.7305)	(0.4884)	(0.3129)
t	-0.1254***	-0.0878***	-0.0895***	-0.0328**	-0.0228	-0.0380***
	(0.0144)	(0.0161)	(0.0128)	(0.0137)	(0.0139)	(0.0115)
Constant	39.4774***	39.2672***	41.1425***	37.1057***	38.3900***	41.2568***
	(0.5409)	(1.1400)	(0.5592)	(0.4864)	(1.1607)	(0.5575)
Observations	21561515	16754427	16754427	21561515	16754427	16754427
R-squared	0.0061	0.1460	0.1946	0.0139	0.1485	0.1961
Controls	No	Yes	Yes	No	Yes	Yes
Country FEs	No	Yes	No	No	Yes	No
County-Sector FEs	No	No	Yes	No	No	Yes

Note: All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index in that year. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016, hours variable is 'usual' hours of work.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the result becomes insignificant (although the result is statistically significant for most of the EU-15 countries in country-level regressions). Overall, there is some evidence that workers in highly non-routine cognitive analytical occupations work slightly more hours over time relative to other occupations with a low non-routine cognitive analytical index. In contrast, the interaction term for high non-routine cognitive personal tasks - such as managerial occupations - is not significantly different from zero across all specifications. It thus appears that such occupations are not experiencing differential trends.

Turning to the analysis of jobs with high routine characteristics, we observe a stark difference in the routine indices between routine cognitive and routine manual occupations (Table 3.2). Individuals working in high routine cognitive occupations have no statistically distinguishable level or trend differences in hours of work relative to others, once covariates are included. However, high routine manual occupations - those most susceptible to automation - have a strongly negative and significant trend interaction term. Such jobs are reducing their hours per worker relative to other jobs. It appears that the well-documented reduction in employment (headcount and total hours) in routine jobs is matched with employment reductions on the intensive margin too, although that appears to be driven by routine manual jobs only.

Non-routine manual jobs also exhibit contrasting patterns between physical and personal. Table 3.3 shows a statistically significant negative interaction trend term for high non-routine manual physical occupations, indicating declining hours relative to other occupations. By contrast, results are not statistically significant for high non-routine manual personal jobs. The latter encompass the service sectors growing (in total employment and total hours) with job polarisation. Our results do not show the same patterns for hours. Using the definition in the online programmes underlying the results in Acemoglu and Autor (2011), the interaction term becomes statistically significant when only country fixed effects are used (Appendix C.4, Table C.10).[8]

Table 3.3: Baseline results for non-routine manual (physical and personal) indices

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	Physical			Personal		
High Index*t	-0.0679**	-0.0658***	-0.0449**	0.0387	0.0017	0.0045
	(0.0287)	(0.0236)	(0.0177)	(0.0305)	(0.0347)	(0.0288)
High Index	4.1315***	1.6831***	0.5127	-4.9628***	-1.6103	-1.2700
	(0.6982)	(0.4911)	(0.3404)	(0.9497)	(1.0041)	(0.9071)
t	-0.0763***	-0.0622***	-0.0683***	-0.1110***	-0.0837***	-0.0866***
	(0.0139)	(0.0152)	(0.0123)	(0.0141)	(0.0151)	(0.0124)
Constant	37.3183***	38.4413***	41.1664***	39.7527***	39.6616***	41.5287***
	(0.4992)	(1.1758)	(0.6041)	(0.3980)	(1.0881)	(0.5212)
Observations	21561515	16754427	16754427	21831786	16959719	16959719
R-squared	0.0197	0.1470	0.1941	0.0240	0.1483	0.1945
Controls	No	Yes	Yes	No	Yes	Yes
Country	No	Yes	No	No	Yes	No
County-Sector FEs	No	No	Yes	No	No	Yes

Note: All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index in that year. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016, hours variable is 'usual' hours of work.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Overall, our baseline results show only some signs supporting the pattern of

[8]If a restriction of limiting our definition to occupations above 299 had not been imposed, the interaction term would have been positive, meaning that non-routine manual personal jobs would display increasing hours per worker in relation to other occupations scoring lower in this index. That result did not seem to be picking up trending hours in well-paid occupations categorised as highly non-routine manual personal. Such occupations include doctors, veterinarians and midwifery professionals – all jobs that require in-person, non-routine care work. To test the sensitivity of the results to these professions, we first exclude them from the analysis, or recategorise them as low non-routine manual personal, and find that the results, if anything, strengthen. In addition, we tested the results for the lower half of the wage distribution, omitting all occupations above the median wage. The interaction between the time trend in hours and the dummy for high non-routine manual personal jobs strengthens again, suggesting that the positive interaction trend term is driven by low-paid high non-routine manual personal jobs. Such jobs are exactly the kind of in person services and care work associated with the Autor and Dorn (2013) polarisation in employment and total hours. Results available upon request.

polarisation in the intensive margin. Hours per worker are declining across all six skill indices, but the slope varies, leading to differences in the evolution in hours per worker across jobs. On the one hand, we see a typical pattern of polarisation when comparing the evolution of hours per worker among non-routine cognitive analytical (declined less than the trend) and routine manual (declined substantially more than the trend), while the pattern for non-routine manual personal is less clear but tends to be not statistically significant. On the other hand, we do not find statistically significant results for non-routine cognitive personal and routine cognitive tasks, while hours declined more than the trend in non-routine manual physical occupations.

Aggregating into three skill categories, we find patterns consistent with polarisation in non-routine cognitive jobs, and in routine jobs. That, however, is not the case in non-routine manual jobs, as we do not find strong evidence in favour of a relative increase in personal jobs, which in any case would be offset by the decline in physical jobs. Our most robust result is thus the strong relative decline in hours per worker in routine occupations driven by routine manual jobs. Additionally, instead of a polarisation pattern we observe that hours declined more in manual jobs (routine manual and non-routine manual physical jobs).

Our results also show that a finer disaggregation of task classification is important to understand hours per worker patterns across occupations. This finding for hours per worker is consistent with the pattern of employment polarisation uncovered by Fonseca et al. (2018) for Portugal. That paper also distinguishes between routine manual and routine cognitive jobs and shows a sharp decline in routine manual employment but only a modest decline in routine cognitive employment. The authors explain the difference between the two routine jobs by the large expansion of the service sector, which employs many workers in routine cognitive jobs.

In the following sections we explore in more detail factors that could help explain our results, beyond (or together with) polarisation associated with technical change. In addition to the analysis we will show in the next sections, we undertook a battery of robustness checks on the main results. First, we replicated all central results with an alternative measure of hours of work: actual hours worked rather than usual hours. Actual hours can vary from usual due to annual leave, sick leave, public holidays and overtime, among other reasons. On average, actual hours are lower than usual hours in levels but the trend remains comparable over time. We replicated the baseline regressions using actual hours of work and found no qualitative changes to the results. In addition, we varied the hours criterion for sample inclusion, at time including or removing observations with zero hours worked, and at others including only those who worked a minimum number of hours (e.g. five hours). These checks yielded broadly the same results as the baseline. Second, we implemented a variety of fixed effects, including country level, industry level and industry-country level interaction. The latter is the most stringent - by retaining only within country industry variation - and as such causes standard errors to increase for some results. The point estimates are, however, essentially unchanged - reassuringly suggesting that failing to remove country-industry fixed effects does not introduce noticeable bias (in the remaining sections we use country-industry fixed effects).

3.4 Potential contributing factors

Routine-biased technical change may be only one of several contributing factors to the patterns in hours per worker uncovered in our baseline results. From 1992-2016, several demographic shifts have taken place in the European labour markets. Most notable are the ageing of the workforce, the increased participation of women, and the increased level of education. In addition, international trade and global supply chains have increased over the same period. Occupations that require little face-to-face interaction have been vulnerable to offshorability, and this may in turn have affected hours of work. Related to both globalisation and technical change, changes in industrial structure and reallocation have occurred over the relatively long period covered in this paper. The distinction between occupational change (jobs and tasks) and industrial change (type of output) is thus important. Moreover, there has been an increasing trend towards part-time work. We consider the impacts of demographic change, offshorability, industrial change and trends in part-time work in turn below.

3.4.1 Demographic trends

We analyse separately three main demographic changes that impacted the European labour markets during the time frame of our analysis: an ageing work force, increased participation of women in the labour market and an ever-more educated workforce.

Age trends: ageing population

Existing work (Autor and Dorn, 2009) finds an increase in the average age of workers in shrinking industries. The outcome is likely a consequence of a standard stock-flow phenomenon – new, younger workers are less likely to enter shrinking industries while older workers, with built-up human capital, remain. From a different perspective, Moreno-Galbis and Sopraseuth (2014) argue that ageing populations can explain the increase in demand for services. Ageing could then be an additional factor that complements technological change in explaining the increase in demand for services via the change in the relative prices of goods and services, as argued by Autor and Dorn (2013). We investigate whether our main results are observed along age and cohort lines. Descriptive statistics from our sample show that individuals work more hours, on average, as they age but there is some concavity to the age profile.

We segment first the sample on both age (at the time of survey) and date of birth. The former compares 25 year olds in 1992 with 25 year olds in 2016, while the latter tracks two different generations – those born between 1953 and 1967, ("Baby Boomers" – oldest aged 65 at end of sample) and 1968 and 1980 ("Generation X" – youngest aged 18 at start of sample) – across time. We then repeat the same regressions from specification 3.1 as for the baseline results. Selected tables are presented in Appendix C.3, Tables C.1-C.3. Within each of the segmented samples the regression results are similar to the overall results and to each other. Put differently, the hours polarisation patterns within age and cohort groups are the same relative to each other and the overall population. The result implies that our baseline results are also observed within age group.

However, the above within-group analysis ignores any group reallocation across occupations. We also regress occupation average age on index specific intercepts and time trends. The results show that occupations with high degrees of routine manual tasks have a lower age than the average occupation but are also growing older over time, relative to other occupations. In short, those occupations have reduced hours of work but increasing average ages. The reverse is true for growing industries – those with high non-routine cognitive tasks and high non-routine personal tasks. In addition to their average hours increasing, their average age is becoming younger.

Taken together, the results suggest that while we do observe shrinking industries "getting old", as per Autor and Dorn (2009), the reallocation across occupations is not driving the hours patterns. Each group is experiencing hours-per-worker patterns trending along routinisation lines.

Gender: increasing female labour force participation

The past two decades have seen a substantial increase in female participation in the labour market. Women accounted for 46% of employment in 2016, up from 41% in 1992, but they have accounted for 70% of the employment growth during this period. At the same time, they work, on average, fewer hours than men (Figure 3.3). A question remains as to whether the increase in female labour participation can explain patterns in hours per worker, rather than just the overall hours trends.

We estimate specification 3.1 for women and men separately for each of the six skill indices. The declining patterns of hours per worker across skill indices do not seem to differ substantially between men and women, but some interesting patterns emerge with the split of the sample. Selected tables are presented in Appendix C.3, Tables C.4-C.6, for the specification with country-sector fixed effects. Within non-routine cognitive jobs, both women and men have a positive interaction term, meaning that hours per worker in these occupations are declining less than the overall declining trend in hours per worker, but the result is never significant. For routine cognitive tasks, there is a statistically significant positive coefficient for men only; for routine manual tasks, instead, the steep overall decline occurs for both men and women, but the coefficient is much stronger for women than it is for men. Similarly, for non-routine manual physical tasks the faster decline in hours worked per person is stronger for women than men. For high non-routine manual personal occupations the positive coefficient of the interaction term is determined by female, but remain not statistically significant. If we instead use the Acemoglu and Autor (2011) definition, then the coefficient is positive for female and negative for male and statistically significant for both (Table C.11).

The decline in hours per worker for women is then an important driving force for the strong decline in hours per worker in routine manual jobs. Taking these results together one can conclude that there may be some additional composition effects from women entering the labour market in certain occupations, but within each group hours per worker patterns tend to conform with the patterns identified for the whole sample.

Education: increasing educational attainment

The most surprising finding of the polarisation literature was that, contrary to previous decades, technological change was not simply skill-biased, but routine-biased; as such, occupational content was a more important predictor of employment loss than skill (captured in years of education). At the same time, it remains true that most employment gains have come at the occupations mostly typical of the highest skilled categories. Educational attainment was steadily increasing over our sample period. Here we examined whether higher educational attainment is behind our baseline results, or whether our baseline results are also observed within each educational category. We split the sample across three education categories: individuals with low education have achieved a lower secondary school education or less; medium education refers to senior secondary school qualification or some tertiary education; and high education refers to an undergraduate degree or above. We then estimate specification 3.1 for each category for each of the six skill indices. As before, this approach does not take into account between group changes but is only suggestive of within group patterns.

The stratification of our sample into three education categories sheds further light on our baseline results. For the non-routine cognitive tasks we find no statistically significant results, meaning that within each group hours per worker decline at broadly the same rate as for other task indices (Table C.7). More importantly – our main result -, the strong decline in routine manual jobs is not driven by education. Hours per worker are declining faster in routine manual jobs than in other jobs for all education groups (Table C.8). By contrast, education does play a role in the observed decline in hours per worker in non-routine manual physical jobs. The estimated decline is driven by high education workers (Table C.9). For non-routine manual personal tasks, results are not statistically significant but the coefficient turns negative for high-education workers. With the Acemoglu and Autor (2011) definition, there is a relative increase in hours per worker for low and medium education while hours per worker decline for workers with high education (Table C.12).

3.4.2 Offshorability

Technological progress, particularly in the area of information and communication, has made it easier to outsource tasks previously performed by middle-skilled workers. In particular, jobs that require little face-to-face interaction, or other on-site requirements, are more at risk of outsourcing. Blinder and Krueger (2013) estimated that about 25% of jobs in the United States could be offshored. Oldenski (2014) found that offshorability has contributed to relative employment gains among high-skilled and relative losses in middle-skilled workers. Goos et al. (2014) have found that offshorability is a contributing, albeit second-order, driver of employment polarisation.

To investigate the impact of offshorability on hours of work, we match the

Table 3.4: Baseline results for offshorability index

	(1)	(2)	(3)
	Hours per worker		
High Index*t	-0.0040	-0.0522**	-0.0375**
	(0.0224)	(0.0228)	(0.0171)
High Index	-0.4611	1.4272***	0.7517*
	(0.5150)	(0.5145)	(0.3980)
t	-0.0990***	-0.0606***	-0.0699***
	(0.0144)	(0.0148)	(0.0112)
Constant	38.8962***	38.9168***	41.0480***
	(0.5409)	(1.1347)	(0.5263)
Observations	21561515	16754427	16754427
R-squared	0.0039	0.1469	0.1941
Controls	No	Yes	Yes
Country FE	No	Yes	No
Country-Sector FE	No	No	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Offshorability is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the offshorability index.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

measure of offshorability created by Acemoglu and Autor (2011) to the EU-LFS dataset using the same series of crosswalks for our six earlier task measures.[9] The index measures the potential for offshoring in an occupation based on the task requirements, rather than the actual degree of offshoring. Analysing the relationship between the offshorability index and average wage we conclude that the potential for offshorability cuts across the entire wage distribution: the quadratic relationship between the offshorability index and average wages is essentially flat with an R-squared value of only 4%.

Similar to the analysis for the task skill indices, we regress hours per worker on an intercept and time trend specific to highly offshorable occupations. Errors are clustered at the country-industry level and a variety of fixed effects and controls are used. The key results, given in Table 3.4, conform to the hours of

[9] Measures of offshorability can vary widely and there is no consensus about the ideal measure. For example, Blinder and Krueger (2013) report three measures of offshorability based on microdata: one self-reported, another being a combination of self-reported measures made internal consistent, and a last and preferable measure by the authors which is based on the assessment of the coders trained by the authors. Using a different approach Firpo et al. (2011) construct three measures based on O*NET. They consider that occupations are more offshorable if: 1) they require little face-to-face communication; 2) they do not require on-site presence; 3) they do not require decision making. Our measure of offshorability based on Acemoglu and Autor (2011) is more closely correlated with the second measure of Firpo et al. (2011).

work polarisation narrative once the full set of controls are added; occupations that have a high degree of offshorability, known to be associated with wage and employment polarisation, have decreasing hours per worker relative to less offshorable occupations. This shows up as a negative and significant coefficient interaction term between time and high offshorability.

In sum, occupations that are highly exposed to technological change and globalisation are experiencing declining average hours. Our results support the view that the intensive margin of employment (hours per worker) seems to be an important adjustment margin for such occupations.

3.4.3 Industrial change

Another important driver of hours per worker could be changing industrial structure, whereby rising sectors tend to be characterised by lower average work hours than contracting sectors. If that is the case, our framework is simply capturing the fact that these rising sectors have a higher composition of expanding skills than contracting skills (i.e. at the poles rather than the middle), and not necessarily the results of divergence across skills per se. While it is not possible to fully disentangle these effects, we attempt to shed light into this issue by separating the within- and between-effects at the sectoral level. In other words, we compare the evolution of average hours from 1992 to 2016 within each 1-digit sector to the shift of employment between sectors, using a standard shift-share analysis.

As the sectoral classification of LFS changed from NACE1 to NACE2 in 2008, it is not straightforward to perform a decomposition for the whole sample, so we instead consider the two subperiods. For the 1992-2007 period, out of a fall of 4% in average (usual) hours worked (from 38.8 to 37.2), 2.8% was attributed to the within-sector component, around 70% of the total. Results for the 2008-2016 period are very similar: approximately 72% of the average (usual) hours decline from 37.1 to 36.3 (1.96%) is a result of a fall within sectors, and only 27% from sectoral shifts. Taking the whole period together, and matching NACE2 to NACE1 sectors (at the letter level) gives a very similar picture, as can be seen in Table 3.5, Panel A.

Overall, the bulk of the hours decline can be accounted for by a decline within each sector, rather than industrial change. This result provides further support to our hypothesis that technological change, manifesting through occupational polarisation, also drives the hours decline. Another way to see this is to relate hours decline for each sector with our task content measures. Panel B of Table 3.5 shows the correlation of the change in usual hours worked over different time

Table 3.5: Sectoral changes of hours worked

(a) Panel A: Shift-share decomposition

Period	Total % change	Within	Between	Interaction	Absolute change	Initial period	Final period
1992-2007	-4.02	-2.82	-0.88	-0.29	-1.56	38.76	37.20
2008-2016	-1.96	-1.41	-0.54	-0.02	-0.73	37.12	36.39
1998-2016	-4.59	-3.41	-0.98	-0.20	-1.75	37.78	36.39
1992-2016	-6.11	-4.19	-1.20	-0.71	-2.37	38.76	36.39

(b) Panel B: Correlation of sectoral hours changes with tasks

	NR Cognitive		Routine		NR Manual		Offshore
	Analytical	Personal	Cognitive	Manual	Physical	Personal	
1992-2007	0.748	0.348	0.420	-0.147	-0.097	0.279	-0.019
2008-2016	0.876	0.506	0.317	-0.307	-0.203	0.449	-0.048
1998-2016	0.756	0.378	0.349	-0.113	-0.063	0.282	-0.059
1992-2016	0.682	0.290	0.289	-0.084	-0.013	0.185	0.007

intervals with the sectoral average of each of our content measures (over the whole period covered). The change in hours worked is most highly correlated with the non-routine cognitive analytical task content, implying that sectors with a high content of such tasks showed the highest increase in hours (equivalently, the lowest reduction). The opposite holds for sectors characterised by a high concentration of routine manual and non-routine manual physical tasks. Indeed, for the 2008-2016 period, the only industries with increasing average usual hours were the ICT sector, utilities, and education, all considered part of the knowledge economy. The ICT sector and education have much higher than average share of occupations with high non-routine analytical content.

3.4.4 Work-time status

Another potential driver of our results may be a rise in part-time employment, a prominent feature of labour markets in Europe during the period studied, as the share of part-time workers in the labour force in the EU-15 countries grew from only 16% in 1995 to 24% in 2016. Of course, the rise of part-time work is itself, at least to some extent, part and parcel of lower average hours. A sufficient reduction in hours may be such that individuals switch from full- to part-time status. That is true whether there is a pre-defined threshold or whether work-time status is self-reported (as it is in the LFS). The forces then that have allowed for workers to work fewer hours on average may have also partially contributed to the rise of part-time work (together of course with changing societal norms, such as higher female participation).

Table 3.6: Full versus part-time status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Hours per worker						
	NR Cognitive		Routine		NR Manual		Offshore
	Analytical	Personal	Cognitive	Manual	Physical	Personal	
Panel A - FT workers							
High Index * t	0.008	0.008	0.027**	-0.037***	-0.026**	0.002	0.027***
	(0.017)	(0.020)	(0.013)	(0.011)	(0.012)	(0.013)	(0.007)
High Index	1.843***	2.330***	-1.871***	0.087	0.314	-0.824***	-0.911***
	(0.403)	(0.461)	(0.262)	(0.243)	(0.298)	(0.303)	(0.218)
t	-0.032***	-0.033***	-0.046***	-0.023*	-0.029**	-0.039***	-0.049***
	(0.008)	(0.008)	(0.013)	(0.012)	(0.013)	(0.011)	(0.012)
Constant	53.37***	53.03***	52.97***	53.19***	52.98***	53.29***	53.08***
	(0.51)	(0.50)	(0.57)	(0.56)	(0.58)	(0.56)	(0.55)
Panel B - PT workers							
High Index * t	0.081***	0.066***	-0.010	-0.206***	-0.106***	0.049**	-0.075***
	(0.022)	(0.022)	(0.018)	(0.026)	(0.030)	(0.020)	(0.019)
High Index	0.039	-0.0687	1.131***	2.866***	1.479***	-0.245	1.594***
	(0.468)	(0.404)	(0.310)	(0.471)	(0.535)	(0.346)	(0.331)
t	0.0112	0.003	0.028*	0.075***	0.044***	0.011	0.051***
	(0.014)	(0.012)	(0.015)	(0.012)	(0.012)	(0.011)	(0.015)
Constant	11.44***	11.30***	11.02***	10.16***	10.86***	10.98***	10.49***
	(0.86)	(0.86)	(0.86)	(0.84)	(0.81)	(0.82)	(0.87)

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. All regressions include controls for age, educational level, sex, size of firm, proxy interview, marital status, and country-sector fixed effects. Industry controls are 1 digit NACE. High Offshorability is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the offshorability index. FT regressions have 13,362,253 observations and PT regressions have 3,344,858 observations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Table 3.6, we repeat our main exercise separately for full- and part-time workers, shown in Panels A and B respectively. For both highly non-routine cognitive occupations, the results are diametrically antithetical. There is a relative increase in hours for part-time workers in these occupations, relative to no effect for full-time workers. This is consistent with the idea that some individuals work fewer hours and drop off the full-time group, raising average hours in the part-time group. The same pattern holds for occupations with high non-routine manual personal content. As shown in the next section, these three task groups are the ones that have gained in employment shares over the past two decades.

By contrast, for the two main losers in employment, routine manual and non-routine manual physical-intensive occupations, average hours exhibit a relative decline for both full- and part-time workers, although the magnitude is much stronger for part-time. For routine cognitive-intensive occupations, in turn, average hours show a relative increase in full-time workers, but no effect for part-time workers. Finally, for the offshorable occupations, the index-trend interaction term is positive and significant for full-time, and negative and significant for part-time.

Overall, for growing occupations, the decline in average hours is driven by a combination of lower hours for full-time workers and a rising share of part-time workers, likely also comprised of formerly full-time workers who work sufficiently less to reclassify their work-time status.[10] For shrinking occupations, both types of workers work fewer hours. The reduction of average hours attributable to lower hours for full-time workers is around ⅓, depending on the initial date.[11] It should be noted that these figures most likely underestimate the within effect. The fluid boundary between part- and full-time work and the likely truncation below of the distribution of hours for full-timers, there is a likely underestimate of the true reduction of full-time hours. Indeed, not only have average hours for part-timers risen, but this increase is driven by higher hours at the upper half of the distribution (75^{th} percentile).

3.5 Employment and hours polarisation?

We have shown that falling hours per worker in Europe exhibit some of the main patterns of RBTC, with large reductions in hours worked for routine manual and some increase in non-routine cognitive analytical (both in relative terms). At the same time, other patterns we found do not necessarily conform to the trichotomy of abstract, routine and manual tasks. Routine cognitive tasks have exhibited little, if any, reduction in hours worked, and non-routine manual physical tasks have exhibited a reduction as large as routine manual. Here we examine how the higher-dimension approach we take compares with what is already established in the literature.

The dashed line in the left panel of Figure 3.5 replicates the analysis of Goos et al. (2014) (henceforth GMS), who show the evolution of employment shares for four low, nine middle and eight high-paying occupations (based on 2-digit ISCO88 classification). The pattern closely resembles the familiar U-shaped pattern identified in GMS qualitatively.[12] The solid line in the left panel

[10]In the absence of panel data, this hypothesis is untestable. However, the hours decline for full-time workers comes from a compression at the right tail; average hours for those working less than 50 hours a week have not changed from 2004 to 2016. Assuming the decline is not limited to the top, it is possible that such behaviour occurs.

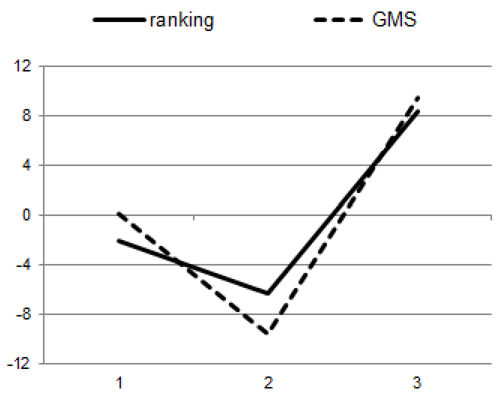
[11]This number is constructed comparing the actual reduction in average hours versus the one that would have occurred had the employment shares of full- and part-time workers remained constant. A shift-share analysis to gauge the importance of each margin is not very informative, since average hours for part-time workers rose during the period studied.

[12]Some quantitative differences remain. Namely, we find no increase at the lower end, while GMS do. There are two possible reasons for this discrepancy. First, our 1992 sample does not include Austria, Belgium, Finland and Sweden; if instead our initial year is 1998, the first year of data for all our 15 countries, the pattern is identical to GSM. Second, GSM use a different

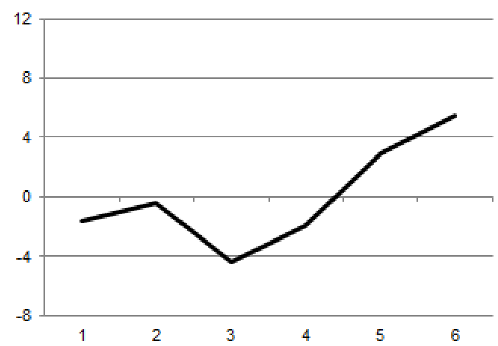
repeats the same exercise, but instead of manually classifying occupations into categories, we group occupations into three quantiles, based on wage ranking in 2008, normalised for each country, with almost identical results.[13] The right panel instead uses a breakdown into six quantiles, and shows that the bottom ⅙ of the wage distribution also experiences a small reduction in employment.

Figure 3.5: Change in employment share by wage category, % share of total employment, 1992-2010

(a) Three categories



(b) Six categories



Source: EU-LFS and authors' calculations. GMS uses the categorization of Goos et al. (2014) into low-, middle- and high-paying occupations. Ranking refers to the classification into quantiles by occupation wage ranking in 2008. The left chart is based on a breakdown into three quantiles, and the right chart into six. Employment is measured by aggregate hours.

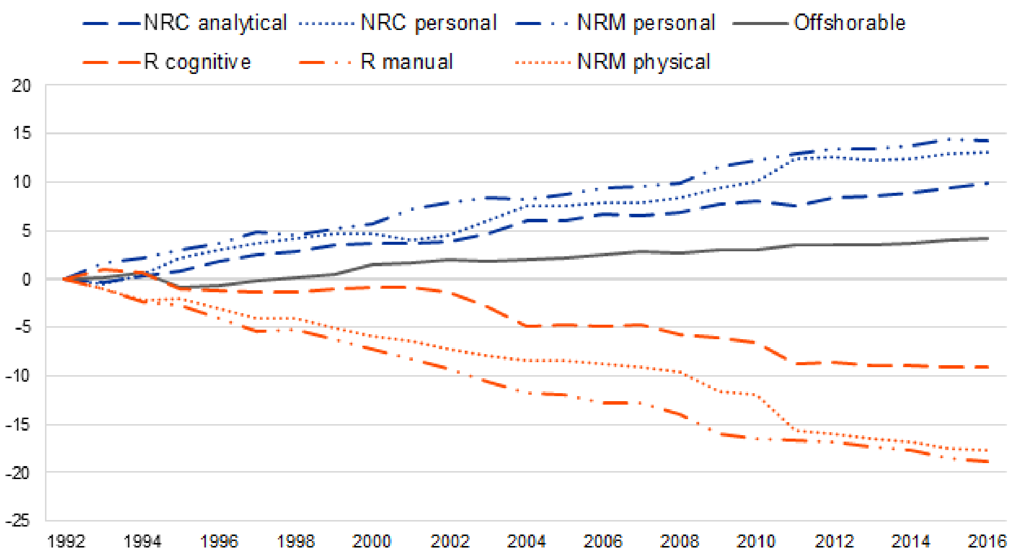
We then examine whether the patterns identified for average hours also hold

sample for Germany. Note that we also include codes 61 and 92 (agricultural and fisheries workers) to the low-wage category for GMS. Without these sectors there is indeed an increase in employment at the low end.

[13]We use EU-SILC for occupational wages, which start in 2004 but have good coverage for all countries in 2008. We do not expect the ranking to fluctuate substantially, and the similarity with the GMS classification is reassuring.

for total employment. Figure 3.6 shows the task content of the mean job in the sample, from 1992-2016, for each of our six categories, plus offshorability, standardized to zero in 1992. Our aim is to identify the overall employment trends without imposing any structure on the classification (as we do in our main analysis where we classify the top third of each task-intensity into our high-content categories). The results are quite stark; the decline of routine manual (and secondarily cognitive) and non-routine manual physical content in the job-pool is substantial. Conversely, equally substantial is the increase in the non-routine cognitive personal (and secondarily analytical) and non-routine manual personal tasks in the content of the average job. While the increasing categories are in line with the trichotomous classification, the decline in non-routine manual physical content is not; there seems to be a stark difference in the employment trends within the non-routine manual category, for physical and personal, for employment levels (shown here) and average hours worked. This is also reflected in the right panel of Figure 3.5; the bottom category, which has exhibited a fall in employment, has a high content of both routine manual and non-routine manual physical tasks.

Figure 3.6: Evolution of task content, 1992-2016 (1992=0)



Source: EU-LFS and authors' calculations. Each line shows the task content of the mean job in the sample, for each task.

The common trends for total employment and average hours across each occupational task index is summarised in Table 3.7, where we show the evolution of total employment shares and average hours for the occupations with a high content of each task, as defined previously. We see that while average hours fell

Table 3.7: Evolution of employment share and average hours

	NR Cognitive		—— Routine ——		—— NR Manual ——		Offshorable
	analytical	personal	cognitive	manual	physical	personal	
	Share of total employment in jobs with high content of each task						
1992	31.5	33.0	30.6	32.6	33.6	15.6	30.1
1998	34.9	37.1	30.2	31.1	32.4	16.5	31.8
2007	39.2	41.2	28.7	27.8	29.3	16.4	33.3
2016	38.7	43.0	30.2	23.1	24.6	19.6	39.6
1992-2010	8.9	10.2	-2.7	-6.9	-6.1	1.6	3.4
1992-2016	7.2	10.0	-0.4	-9.5	-9.0	4.0	9.5
	Average hours in jobs with high content of each task						
1992	40.3	39.1	37.2	40.9	40.9	35.4	38.0
1998	40.1	39.9	36.9	40.3	40.5	34.3	37.8
2007	39.7	39.3	36.8	39.3	39.4	32.8	37.0
2016	38.4	36.9	36.0	36.1	37.8	33.7	35.8
1992-2010	-0.9	-0.1	-0.8	-3.8	-2.1	-2.5	-0.9
1992-2016	-1.9	-2.2	-1.2	-4.8	-3.2	-1.7	-2.2

across tasks, they fell more for those tasks that suffered employment losses. It should be noted that when calculating the total employment shares, we fix the task content cut-offs at their levels in 1992; we cannot let the cut-off vary by year, otherwise there would not be meaningful variation in employment shares across time. On the other hand, the cut-off for average hours does vary by year; since fewer jobs had, for instance, high non-routine cognitive analytical content in 1992 than in 2016, using a fixed cut-off may classify into the high non-routine cognitive analytical content category jobs that we would not necessarily consider as possessing this attribute.[14] Conversely, shrinking occupations, such as those with high routine manual content may not be classified as such using a fixed cut-off, even though they would meet conventional criteria to be classified as such.[15]

We then consider how average hours evolve by wage categories, to gauge whether the U-shaped pattern of employment holds. The left panel of Figure 3.7 repeats the analysis of Figure 3.5 for the GMS classification and the ranking based on wages by occupation in 2008, for three quantiles. We see that in this case, the choice of classification does play a role in the results. In the GMS classification, there is a hump-shaped response: the fall in hours is higher for the lowest-paid occupations, then for the highest, and the smallest reduction is shown by the middle categories. However, using a quantile-based ranking using three

[14] Examples include medical technicians, technical/medical sales professionals, credit and loan officers, insurance and sales representative, broadcasting technicians, travel consultants, and electrical installers and servicers.

[15] Examples include bank tellers and related clerks, cashiers and ticket clerks, vehicle repairers.

quantiles, we see an inverse L-shaped pattern, with the losses in average hours being monotonically negatively related to wages. As such, while employment may be polarising, with more jobs created at high- and low-wage occupations, average hours are falling more in low-wage occupations.

As it appears that the results for average hours are sensitive to the wage classification, we further consider the distribution of hours losses using a six quantile wage grouping in the right panel of Figure 3.7, which shows an inverse U-shaped pattern of average hours losses until the 83^{rd} percentile and lower losses thereafter. The overall conclusion remains with finer categorisations, namely that, by and large, there is no polarisation in hours losses, but rather lower losses at higher wages. That is, while employment gains are characterized by a U-shaped pattern, hours losses are characterized by an inverse L-shaped pattern.

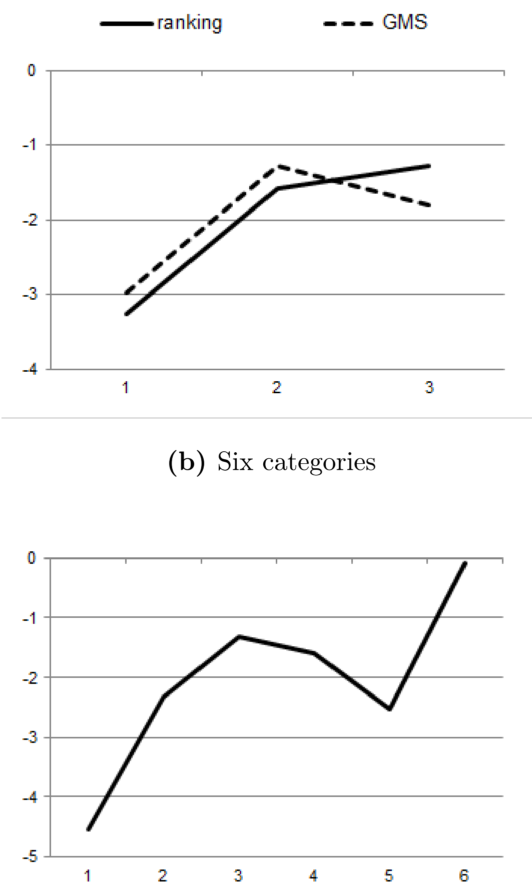
Since the literature typically considers total hours as a measure of total employment when considering polarisation, it follows that this pattern is at least partially driven by changes in the patterns of average hours. An interesting exercise is to consider whether the polarisation pattern would differ were it not for the change in the hours pattern. Figure 3.8 repeats the exercise of Figure 3.5 but now considers the wage ranking classification for an aggregate employment measure based on heads, as well as total hours. While both of these broadly follow the same pattern, the main differences are in the low- and high-wage categories. For the low-wage category, the change in hours-based employment share is almost 2 percentage points (pp) lower than in the heads definition, reflecting the fact that average hours have fallen for routine manual and non-routine manual physical tasks. For the high category, higher average hours reflect a gain in employment share of over one percentage point more with the total hours measure.[16] The six-quantile ranking reveals that the additional gain at the top with the total hours measure is driven by the very top, where the employment share gain is over 1.2 percentage points higher with the total hours measure.

3.6 Country comparisons

In this section we analyse country specific patterns on the polarisation of hours worked per person. First, we analyse whether the results obtained for the aggregation of the EU-15 countries hold for most of the individual countries. Then, we

[16]Absolute differences are less important, as they are quite sensitive to the classification used. With the updated version of the GMS classification defined above, a 2.1 percentage point gain in employment share for the low group with the heads definition is 0.1 with the total hours definition. With the original GMS classification these figures are 3.5 and 1.9, respectively.

Figure 3.7: Change in average hours by wage category, 1992-2010



(a) Three categories

(b) Six categories

Source: EU-LFS and authors' calculations. GMS use the categorization of Goos et al. (2014) into low-, middle- and high-paying occupations. Ranking refers to the classification into quantiles by occupation wage ranking in 2008. The left chart is based on a breakdown into three quantiles, and the right chart into six. Employment is measured by aggregate hours.

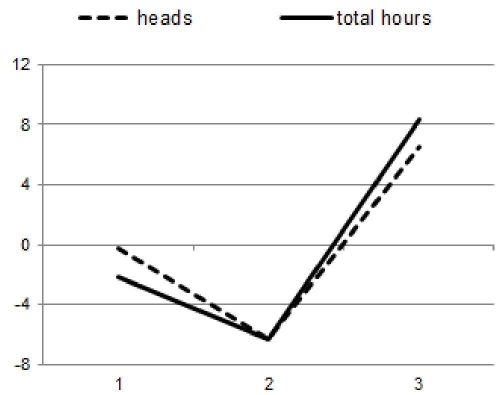
analyse whether the patterns uncovered for the EU-15 countries are also observed for the United States.

3.6.1 Individual EU-15 countries

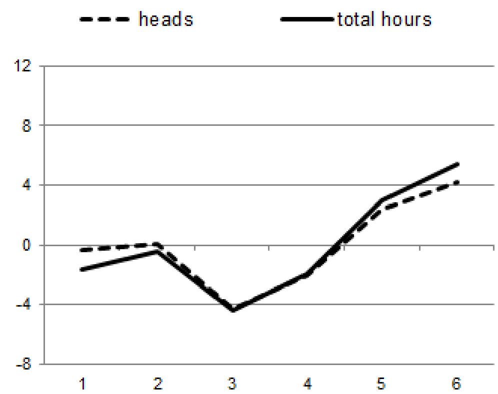
The level and developments of hours worked per person across the EU-15 countries are very heterogeneous (e.g. Ohanian and Raffo 2012). Thus, it is important to carry out a country level analysis to determine whether the results obtained for the EU-15 are common to most individual countries or are driven just by a few. We carry out the same analysis as for our baseline results to each of the EU-15 countries. Here we focus on our baseline results relating to non-routine cognitive

Figure 3.8: Employment growth by wage category, % share of total employment, 1992-2010

(a) Three categories



(b) Six categories



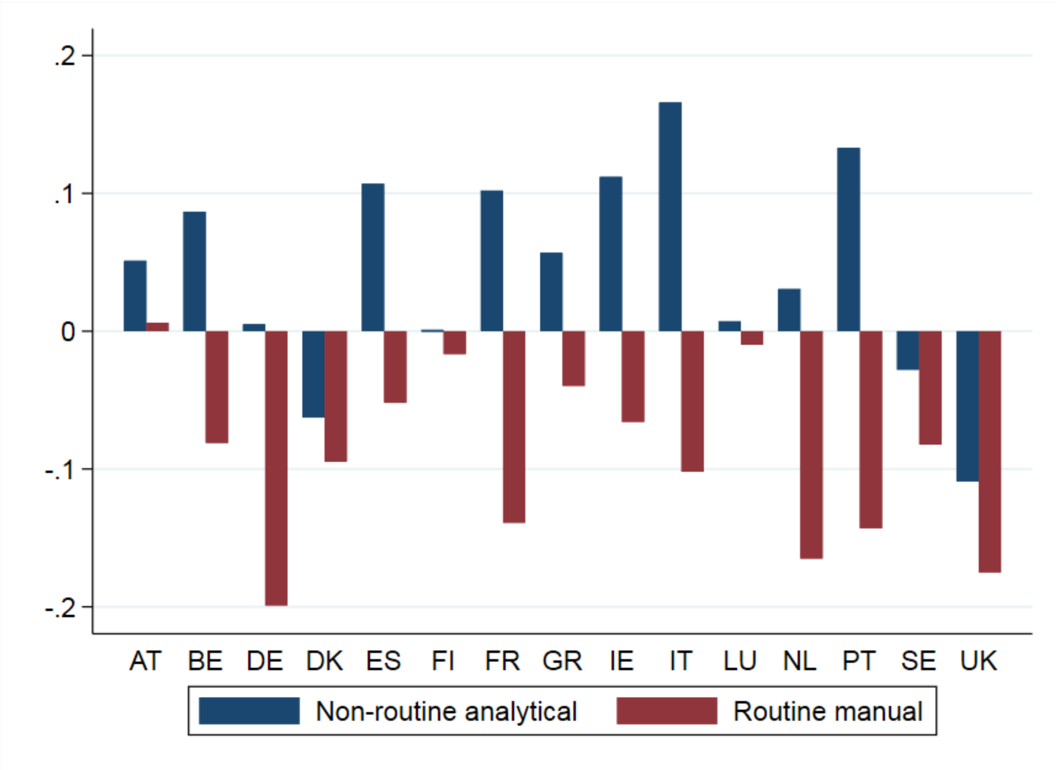
Source: EU-LFS and authors' calculations. The left chart is based on a breakdown into three quantiles, and the right chart into six. Total hours refers to employment being measured by aggregate hours, while heads uses a headcount measure.

analytical jobs and routine manual jobs. Table C.13 in Appendix C.5 shows the results for all six indices and offshorability.

The results described in Section 4 are broadly consistent across countries. A visual representation is very helpful to summarise the main results by country. Hours per worker in routine manual jobs are declining faster than trend hours in all countries with the exception of Austria. For non-routine cognitive analytical jobs, hours per worker are increasing in most countries with the exception of Denmark, Sweden and the UK, while estimates are not statistically significant for Germany, Finland and Luxembourg (Figure 3.9).

This results show that our baseline results are observed for a large set of countries and not driven by just a set of a few countries. In particular, the decline

Figure 3.9: Country specific results



Source: EU-LFS and authors' calculations. All results statistically significant with exception of DE, FI and LU for non-routine analytical tasks.

in hours per worker in routine manual jobs is very robust. However, more research is necessary to uncover the reasons for different patterns across countries for the other indices.

3.6.2 United States

We extend our analysis to compare the EU-15 hours per worker across occupational task indices with the US labour market. The US has not experienced the large decline in average hours of work since the Great Recession, in contrast to Europe. It has, however, exhibited well documented employment and wage polarisation. The lack of a decline in average hours does not, by itself, preclude hours polarisation: routine jobs may reduce their average hours but be outweighed by high skilled and low-skilled hours increases.

We repeat our analysis for the US using the Current Population Survey (CPS) – the US equivalent to the EU-LFS. Interestingly enough, the baseline results presented in Tables 3.8, 3.9 and 3.10 uncover the polar opposite to the EU-15

Table 3.8: US results for non-routine cognitive (analytical and personal) indices

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	Analytical			Personal		
High Index*t	-0.0296**	-0.0241**	-0.0430***	-0.0230	-0.0186	-0.0287**
	(0.0141)	(0.0104)	(0.0090)	(0.0170)	(0.0141)	(0.0127)
High Index	5.0834***	3.2788***	3.3540***	5.0665***	3.7217***	4.3557***
	(0.1891)	(0.1291)	(0.1190)	(0.2217)	(0.1697)	(0.1508)
t	-0.0445***	-0.0641***	-0.0498***	-0.0471***	-0.0675***	-0.0494***
	(0.0135)	(0.0099)	(0.0076)	(0.0168)	(0.0122)	(0.0086)
Constant	37.6631***	33.1511***	36.0557***	37.6752***	33.0519***	36.1736***
	(0.1971)	(0.4420)	(0.4716)	(0.2306)	(0.4857)	(0.4691)
Observations	1683460	1641808	1641808	1683460	1641808	1641808
R-squared	0.0288	0.0902	0.1082	0.0294	0.0951	0.1179
Controls	No	Yes	Yes	No	Yes	Yes
State FEs	No	Yes	Yes	No	Yes	Yes
Sector FEs	No	No	Yes	No	No	Yes

All regressions weighted with CPS weights, and standard errors clustered at state-sector level. Controls: age, educational level, sex, size of firm, marital status. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index in that year. Sample is individuals working non-zero hours in the CPS sample from 1995-2016.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.9: US results for routine (cognitive and manual) indices

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	Cognitive			Manual		
High Index*t	0.0723***	0.0606***	0.0387***	0.0115	0.0181	0.0390***
	(0.0101)	(0.0101)	(0.0083)	(0.0167)	(0.0132)	(0.0114)
High Index	-2.3558***	-1.2937***	-1.9043***	-1.4834***	-1.1819***	-1.8206***
	(0.1261)	(0.1341)	(0.1077)	(0.2475)	(0.1832)	(0.1415)
t	-0.0791***	-0.1009***	-0.0868***	-0.0582***	-0.0843***	-0.0850***
	(0.0134)	(0.0098)	(0.0062)	(0.0104)	(0.0087)	(0.0063)
Constant	40.2408***	33.5315***	36.9692***	39.9215***	34.0500***	37.9422***
	(0.1917)	(0.4641)	(0.4692)	(0.1524)	(0.4564)	(0.4770)
Observations	1683460	1641808	1641808	1683460	1641808	1641808
R-squared	0.0039	0.0824	0.1034	0.0029	0.0826	0.1026
Controls	No	Yes	Yes	No	Yes	Yes
State FEs	No	Yes	Yes	No	Yes	Yes
Sector FEs	No	No	Yes	No	No	Yes

All regressions weighted with CPS weights, and standard errors clustered at state-sector level. Controls: age, educational level, sex, size of firm, marital status. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index in that year. Sample is individuals working non-zero hours in the CPS sample from 1995-2016.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

results. Highly routine occupations - both routine cognitive and routine manual - have experienced increasing hours trends relative to other occupations. This is in direct contrast to the strongly negative routine manual results for EU-15 countries. Also contrasting the EU results are the negative and significant interaction trends for high skilled, non-routine analytical occupations and the lower-skilled non-

Table 3.10: US results for non-routine manual (physical and personal) indices

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	Personal			Physical		
High Index*t	-0.1323***	-0.0676***	-0.0357***	-0.0264**	-0.0177*	-0.0130
	(0.0114)	(0.0100)	(0.0097)	(0.0128)	(0.0103)	(0.0088)
High Index	1.9452***	1.2127***	1.9347***	0.4366**	0.1581	-0.5700***
	(0.1628)	(0.1434)	(0.1397)	(0.1810)	(0.1573)	(0.1341)
t	-0.0092	-0.0568***	-0.0571***	-0.0457***	-0.0748***	-0.0690***
	(0.0130)	(0.0083)	(0.0052)	(0.0133)	(0.0093)	(0.0066)
Constant	38.7755***	32.8718***	36.4226***	39.2958***	33.1101***	37.3402***
	(0.1794)	(0.4541)	(0.4667)	(0.1963)	(0.4720)	(0.4928)
Observations	1683460	1641808	1641808	1683460	1641808	1641808
R-squared	0.0019	0.0822	0.1032	0.0008	0.0818	0.1013
Controls	No	Yes	Yes	No	Yes	Yes
State FEs	No	Yes	Yes	No	Yes	Yes
Sector FEs	No	No	Yes	No	No	Yes

All regressions weighted with CPS weights, and standard errors clustered at state-sector level. Controls: age, educational level, sex, size of firm, marital status. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index in that year. Sample is individuals working non-zero hours in the CPS sample from 1995-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

routine manual personal occupations. Both of these had positive hours trends relative to other occupations in the EU.[17]

The results are robust to a variety of clustering and fixed effects specifications (industry, state, industry-state interactions), and sample inclusion criteria (strictly positive hours worked, zeros, and minimum hours).

The contrasting results between the US and Europe may not be too surprising. We posit that the differences are possibly due to employment regulations governing firms' labour adjustment. US employment law does not contain the same labour protections, particularly with regard to firing restrictions, as European employment law. As a result, it is less costly for American firms to reduce headcounts of workers in shrinking industries. European firms, by contrast, must perhaps rely more on intensive margin hours adjustments. An additional hypothesis possibly underlying the results is the marketisation of home production which occurred earlier in the US and explains much of the EU-US employment and hours gap (e.g. Freeman and Schettkat 2005).

[17]For non-routine manual personal tasks the results for the US comprise all occupations while for the EU-15 they concern only ISCO codes above 299.

3.7 Conclusion

In this paper we analysed whether hours per worker were an additional margin of employment polarisation. Our results suggest a relation between hours per worker and employment polarisation patterns. The level and, more importantly, the trend in hours per worker vary considerably across each occupational task index. This is a new fact that has not been previously considered in the polarisation literature.

We find large declines in routine manual jobs – precisely the occupations most negatively affected by employment polarisation from RBTC. Additionally, we find a lower decline in hours per worker for non-routine cognitive analytical jobs, which are growing through polarisation. At the same time, hours per worker declined significantly more than the trend for non-routine manual physical occupations and that decline has not been compensated by an increase in hours per worker in non-routine manual personal jobs. However, for non-routine manual jobs the occupational task indices also do not give the typical polarisation patterns for total employment. Overall, instead of a polarisation pattern our results show that hours per worker declined more in manual jobs (routine manual and non-routine manual physical).

Our results remain robust to estimation across age, gender and education groups, although the intensity may vary and some subtle patterns may emerge. For example, the decline in hours per worker in routine manual jobs and non-routine manual physical jobs is stronger for women. The decline in hours per worker occurs mostly within sectors. The increase in part-time seems important. However, that increase may be partly a consequence of the decline in hours per worker, as the classification into full-time and part-time is self-reported.

Using a wage ranking of occupations instead of the occupational task indices, the decline in hours per worker is monotonically related to wages. First, the results we obtained for employment changes using a wage ranking is a U-shaped curve, in line with the empirical literature. However, when the wage ranking is divided in six quantiles instead of three, we observe that the bottom quantile experiences employment losses similar to the middle. Second, hours per worker appear monotonically related to wages: using three quantiles of the wage ranking of occupations we observe a sharper decline in the bottom quantile, a milder decline in the middle and almost no decline at the top; using six quantiles for the wage ranking of occupations we observe an inverse U-shaped pattern for most of the distribution, but with lower decrease in hours per worker in the top quantile. Thus, while employment gains are characterised by a U-shaped pattern, the decline in hours per worker is characterised by an inverse L-shaped pattern.

Taken together, these results suggest that patterns in hours per worker exacerbate the impact of employment polarisation on wage inequality. High-skilled workers increased their fraction of employment and work relatively more hours, medium-skilled workers saw a decline in the share of employment and a decline in hours per worker and low-skilled workers saw a substantial decrease in hours per worker. The analysis based on the wage ranking of occupations makes this point even clearer; hours declined significantly more in low-paying occupations.

The patterns in hours per worker uncovered for the EU-15 aggregate are observed in most of each of the individual countries. The results for the United States are fundamentally different. We tentatively suggest that labour market institutions can play a role. In addition, the earlier marketisation of household production in the United States can additionally help to explain the differences found between the EU-15 and the Unites States.

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Appendix A

Appendix for Chapter 1

A.1 Search theory background

A.1.1 Baseline search model

In a standard search model, a la Pissarides (1990, 2000), unemployed workers searching for a job receive a flow value characterised by the following Bellman equation:

$$\rho V_U = b + \lambda \int_{\rho V_U} [V_E(w(\theta)) - V_U] dF(\theta) \tag{A.1}$$

Where b is an unemployment benefit, ρ the discount rate, and λ the probability that the worker is matched to an employer. Job offer matches have heterogeneous productivities, θ following distribution $F(\theta)$. Workers only accept employment offers that yield higher utility than their current state. As a result, there is some critical productivity value, $\hat{\theta} = \rho V_U$, for which all matches at least as great as $\hat{\theta}$ are accepted.

Workers employed at a job have the following flow value:

$$\rho V_E = w(\theta) + \eta(V_U - V_E((\theta))) \tag{A.2}$$

They receive a wage that is specific to the match productivity θ. With some exogenous probability η the match is terminated and the worker becomes unemployed.

Firms with a filled position receive the productivity value of the match, θ, pay wages $w(\theta)$ and face the same exogenous probability η of a terminated contract.

$$\rho V_F = \theta - w(\theta) - \eta V_F \tag{A.3}$$

Vacancies cost c and are filled with probability λ_V. Free-entry of vacancy creation is assumed, pushing the value unfilled vacancies to 0:[1]

$$\rho V_V = -c + \lambda_V(V_F - V_V) = 0 \tag{A.4}$$

The number of matches is a constant returns to scale matching technology that depends positively on the stocks of unemployed workers, u, and vacancies, v.

$$M(u,v) = M(\frac{u}{v}, 1) = vq(k) \tag{A.5}$$

where $k = \frac{u}{v}$ and the partial derivatives are positive $\frac{\partial M}{\partial u}, \frac{\partial M}{\partial v} > 0$.

Wages are determined through Nash bargaining between workers and firms where α is the relative bargaining power of workers.

$$w(\theta) = \underset{w}{\operatorname{argmax}}[V_E - V_U]^{\alpha}[V_F]^{1-\alpha} \tag{A.6}$$

The system of the equations can be solved for the equilibrium unemployment level u:

$$u = \frac{\eta}{\eta + (1 - F(\hat{\theta}))q(k)/k} \tag{A.7}$$

A.1.2 Search intensity

Search intensity is a primary features of interest in the paper. To model search intensity, the job finding rate of a worker becomes a function of their search effort s_i, the aggregate search effort of all job seekers and the number of vacancies.[2]

$$\lambda_i = \frac{s_i M(su, v)}{su} = M(s, \frac{v}{u}) \tag{A.8}$$

The job finding rate is increasing in individual search and decreasing in aggregate search, i.e. $\frac{\partial \lambda_i}{\partial s_i} > 0$ and $\frac{\partial \lambda_i}{\partial s} < 0$. Additional search effort is costly for the individual. The cost of search, $\sigma_i(s_i)$, is modelled as a strictly increasing and convex function to ensure an interior solution.

$$\sigma_i(s_i) \text{ where } \sigma_s > 0, \sigma_{ss} \geq 0 \tag{A.9}$$

[1]The probability the vacancy is filled is the product of the probability of a match (see equation A.5) times the probability the job offer is accepted $1 - F(\rho V_U) = 1 - F(\hat{\theta})$, i.e. the probability that the productivity of the match, θ exceeds an unemployed workers reservation level

[2]As per Pissarides (2000).

The value function for an unemployed worker is adapted to take into account the search intensity tradeoff. For a simplification that demonstrates the essence of the problem, all job matches are assumed homogeneous. There is therefore a single value V_E for employment and a single wage w.

$$\rho V_{Ui} = b - \sigma(s_i) + \lambda(s_i, q)(V_E - V_{Ui}) \tag{A.10}$$

The optimal search intensity maximises the value of unemployed search, trading off the gains of greater search (increased job finding rate) with the increased cost. The first order condition (applying the envelope theorem) is:

$$-\sigma_s(s_i) + \frac{\partial \lambda(s_i, q)}{\partial s_i}[V_E - V_{Ui}] = 0 \tag{A.11}$$

The partial derivative of λ is evaluated by using the functional form assumption from equation A.8 and imposing a symmetric equilibrium $s_i = s$ as follows:

$$\left.\frac{\partial \lambda_i}{\partial s_i}\right|_{s_i=s} = \frac{\lambda_i}{s} \tag{A.12}$$

Finally we can substitute the term for $V_E - V_{Ui}$ into the first order condition to obtain an expression for optimal search intensity:

$$-\sigma_s(s_i) + \frac{w - b + \sigma(s_i)}{\rho + \eta + \lambda(s_i, q)} \frac{\lambda(s_i, q)}{s} = 0 \tag{A.13}$$

Two possible effects of minimum can be observed through this equation. Firstly, search effort is increasing in the offered wage. Minimum wages that raise the offered wage provide a stronger incentive to find employment, increasing the return to search. Secondly, however, search effort is decreasing in the ratio of vacancies to unemployed searchers. If minimum wages increase the number of searching individuals, v/u falls, generating a congestion externality and decreasing search effort of existing searchers.

A.1.3 On-the-job search

When including on-the-job search, the stock of job seekers is now the sum of unemployed workers, u and workers searching on-the-job e. The matching function therefore becomes:

$$M = M(u + e, v) = M(\frac{u + e}{v}, 1) = vq(k) \tag{A.14}$$

Where v is the stock of vacancies, u is the stock of unemployed workers and e

the stock of employed workers searching for a new job. Inverse market tightness is now $k = \frac{u+e}{v}$. For analytical tractability, it is assumed that unemployed and employed job seekers contribute equally to the matching function and have the same job finding rates.[3] The job offer rate for all job seekers is therefore $\lambda = M(u+e,v)/(u+e) = q(k)/k$.

Employed workers choose whether or not to search on-the-job. Searching provides the chance to switch to a higher productivity job but also incurs a direct cost of search, σ. These two features are traded off in the workers decision to search or not. The value function for searching, superscripted s, in job of productivity θ is:

$$\rho V_E^s(\theta) = w^s(\theta) - \sigma + \lambda \int_\theta (V_E(x) - V_E^s(\theta)) dF(x) + \eta(V_U - V_E^s(\theta)) \qquad \text{(A.15)}$$

λ is the probability of a new job offer. Only offers from jobs of greater productivity are accepted, i.e. $x > \theta$. Therefore with probability $\lambda(1 - F(\theta))$ on-the-job search results in a job-switch and resulting value change $V_E(X) - V_E^s(\theta)$. As before, a match may be terminated for exogenous reasons with probability η.

If a workers opts not to search they save the cost σ but lose the opportunity to switch to a better job. The value function, superscripted ns, in such cases is:

$$\rho V_E^{ns}(\theta) = w^{ns}(\theta) + \eta(V_U - V_E^{ns}(\theta)) \qquad \text{(A.16)}$$

Workers in job θ will choose to search on-the-job if the benefits of doing so outweigh the costs. The benefit is the expected gain from a new job multiplies by the probability it occurs. The cost constitute the direct search cost σ and any wage differential $w^{ns}(\theta) - w^s(\theta)$.[4]

$$\lambda \int_\theta (V_E(x) - V_E^s(\theta)) dF(x) \geq w^{ns}(\theta) - w^s(\theta) + \sigma \qquad \text{(A.17)}$$

The first order implication of binding minimum wages is the reduction in the value of switching jobs arising from compression in the wage distribution. For jobs with low values of θ, where the minimum wage binds, the current employment value is higher, and the expected gain from a new job is lower. As a consequence we are likely to see fewer job-to-job transitions in particular from low-productivity

[3] Pissarides (2000)

[4] In Pissarides (2000), wages are higher for non-searchers than searchers because searching imposes the cost of a potential quit on the firm. Firms observe whether their workers are searching so can adjust the wage to recoup some of this cost.

jobs. Phrased differently, there is concern that minimum wages disrupt the start of the job-ladder model.

A.2 Classifying active versus passive search

Active measures

1. Visit a Job Centre
2. Visit a Careers Office
3. Visit a Jobclub
4. Advertise in newspapers or journals
5. Answer job advertisements
6. Apply directly to employers
7. Look for premises or equipment (self-employment)
8. Seek any kind of permit (self-employment)
9. Try to get a loan or other financial backing (self-employment)

Passive measures

1. On books of a private employment agency
2. Study situations vacant in newspaper or journals (but not answer)
3. Wait for results of application for job
4. Ask friends, relative, colleagues or unions
5. Do anything else to find work

A.3 Additional descriptive statistics

Table A.1: Position in the wage distribution prior to 2010 minimum wage change

	Age	21	21-23	18-25	16-64
Below	%	0.66	0.75	1.43	0.91
	N	18	68	334	1,547
Youth Spike	%	2.62	0.87	2.96	0.53
	N	71	79	691	898
Between	%	7.24	2.85	5.34	1.29
	N	196	259	1,249	2,197
Adult Spike	%	11.08	10.78	10.03	4.34
	N	300	979	2,344	7,396
Above	%	78.39	84.75	80.24	92.94
	N	2,122	7,697	18,750	158,534
Total	%	100	100	100	100
	N	2,707	9,082	23,368	170,572

Table discretises the wage distribution into five groups: those earning below the youth minimum wage, those earning within ±2% of youth minimum wage, those earning between 2% above youth minimum wage to 2% below adult minimum wage, those earning within ±2% of adult minimum wage and those earning more than 2% above adult minimum wage. Sample: individuals within 24 months of Oct 2010. Spike means earning within 2% of respective minimum wage. Source: ASHE 2010.

Table A.2: Descriptive statistics for search intensity

		Type of Searching		No. methods	
		Passive	Active	1	2-14
Unemployed search	N	3,963	3,105	6,410	658
	%	56.07	43.93	90.69	9.31
OJS	N	3,362	1,167	4,232	297
	%	74.23	25.77	93.44	6.56
Total	N	7,325	4,272	10,642	955
	%	63.16	36.84	91.77	8.23

Sample: 21-23 year olds, 24 months either side of 2010 policy change, searching for a job

A.4 Additional results for non-employed search

A.4.1 Additional baseline estimates

Table A.3 presents additional baseline estimates for the impact of higher minimum wages on extensive margin non-employed search. The table includes students in the regressions, including as a separate dependent variable category, and has no controls. To meet statistical disclosure requirements of the UK Data Service, the constant is withheld.

Table A.3: Unemployed search estimates, no controls, including students

	(1) Working	(2) Unemployed	(3) Inactive	(4) Student
Post	-0.0114*	0.00736*	0.00807*	-0.00402
	(0.00675)	(0.00395)	(0.00439)	(0.00456)
Age 21	-0.0877***	0.00798	-0.00784	0.0876***
	(0.0180)	(0.00637)	(0.00591)	(0.0148)
Post*Age 21	-0.0144	0.0153**	-0.0142**	0.0133
	(0.0127)	(0.00708)	(0.00644)	(0.00927)
Constant	[Witheld]			
Observations	53381	53381	53381	53381
Controls	No	No	No	No

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. The constants are witheld to conform to UK Data Service statistical disclosure control requirements.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4.2 Sub-population analysis

An amount of analysis was undertaken to test if the treatment estimates varied by educational attainment and region of residence. One would expect minimum wage policy to impact less educated groups more strongly than highly educated groups. Individuals were classified as high-education or low-education using QLFS information on their highest qualification. A variety of different definitions were constructed for robustness checking and all gave broadly the same results. The results presented here define low education as individuals with a highest qualification of A-level and equivalent or below and no post-school education.

High education is considered to be individuals with any post-school education (i.e. above A level).

Two main approaches for testing for differential treatment estimates were used. Firstly, educational attainment was interacted with the treatment interaction term (with the education variable also included as a first order effect) using the whole sample of 21-23 year olds. Secondly, the sample was stratified into high and low education individuals and separate difference-in-differences regressions were run. Both methods gave the same story: the headline results appear to be driven by low education individuals. No statistically significant treatment effects are estimated for highly educated individuals. The second approach, using split samples, is more easily interpretable and so is shown here in Table A.4.

A second additional formulation brings geography into the equation - one would expect the same minimum wage to have more significant consequences in a low wage area relative to a high wage area. The QLFS and ASHE both include numerous geographic classifications. Two sets of geographic delineations are used here: travel-to-work-areas (TTWAs), commonly considered the best measure of a local labour market in the UK, and NUTS Level 2 (Nomenclature of Units for Territorial Statistics).[5] For a given set of geographical classifications, the ASHE is used to calculate the mean and median regional hourly income.[6] The local Kaitz index - the adult minimum wage divided by local average hourly wage - is calculated for each region.

Geographic variation is used in two ways, similar to the education investigation above. Firstly, I interact the local Kaitz index with the treatment interaction term ($Treatment * Age21$) and include the relevant first order effect separately. Secondly, I stratify the sample into those individuals living in below average income areas and those in above average income areas. Both approaches give the same results: no statistically significant differences in treatment effects are estimated based on regional income.

[5]TTWAs are defined as: at least 75% of the area's resident workforce work in the area, and at least 75% of the people who work in the area also live in the area. There are over 200 TTWAs and some contain very small numbers of sampled individuals. As a consequence, the ASHE calculates average income with a non-negligible degree of sampling error in the small TTWAs, introducing noise into the estimates. In response, I also use a second, larger geographic definition, NUTS2. NUTS2 categorises the UK into 39 separate regions in 2010 (6 for Scotland, 2 for Wales, 1 for Northern Ireland and 30 for England).

[6]Two measures of hourly income are used: all paid income divided by paid hours worked excluding overtime and all paid income divided by hours worked including overtime.

Table A.4: Unemployed search estimates by education levels

	Low education individuals			High education individuals		
	(1)	(2)	(3)	(4)	(5)	(6)
	Working	Unemp	Inactive	Working	Unemp	Inactive
Post	-0.0263***	0.0129**	0.0134**	-0.00241	0.00136	0.00105
	(0.00847)	(0.00564)	(0.00620)	(0.00913)	(0.00750)	(0.00748)
Age 21	-0.0144	0.0150**	-0.000604	-0.0733***	0.0627***	0.0107
	(0.00983)	(0.00690)	(0.00707)	(0.0163)	(0.0138)	(0.00906)
Post*Age 21	0.00239	0.0189*	-0.0213**	-0.0139	0.0208	-0.00690
	(0.0131)	(0.00976)	(0.00954)	(0.0220)	(0.0211)	(0.0143)
Constant	0.704***	0.193***	0.103***	0.737***	0.178***	0.0849***
	(0.0383)	(0.0179)	(0.0283)	(0.0318)	(0.0265)	(0.0189)
Observations	33463	33463	33463	11685	11685	11685
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change split by educational attainment. Columns 1-3 are for low education individuals, columns 4-6 for high education individuals. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Unemployed search estimates by regional income - NUTS2 regions

	—— Poorer regions ——			—— Richer regions ——		
	(1)	(2)	(3)	(4)	(5)	(6)
	Working	Unemp	Inactive	Working	Unemp	Inactive
Post	-0.0218**	0.00658	0.0152**	-0.0210**	0.0148**	0.00620
	(0.00878)	(0.00578)	(0.00656)	(0.00847)	(0.00660)	(0.00627)
Age 21	-0.0435***	0.0304***	0.0131*	-0.0180*	0.0226**	-0.00455
	(0.0113)	(0.00833)	(0.00780)	(0.0105)	(0.00910)	(0.00658)
Post*Age 21	0.00508	0.0177	-0.0228**	-0.0145	0.0224*	-0.00792
	(0.0139)	(0.0114)	(0.0105)	(0.0174)	(0.0127)	(0.0108)
Constant	0.572***	0.236***	0.192***	0.596***	0.174***	0.230***
	(0.0253)	(0.0149)	(0.0207)	(0.0319)	(0.0189)	(0.0247)
Observations	21701	21701	21701	21408	21408	21408
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change, split by regional income in 2010. Income measured median hourly wage, excluding overtime, of each NUTS2 region (from ASHE). Poorer (richer) regions are the 50% of NUTS2 regions with the lowest (highest) median hourly wage. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, education, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Unemployed search estimates by regional income - TTWA regions

	—— Poorer TTWAs ——			—— Richer TTWAs ——		
	(1)	(2)	(3)	(4)	(5)	(6)
	Working	Unemp	Inactive	Working	Unemp	Inactive
Post	-0.0230**	0.00900	0.0140*	-0.0194*	0.0118	0.00759
	(0.00973)	(0.00657)	(0.00775)	(0.0101)	(0.00826)	(0.00713)
Age 21	-0.0325***	0.0282***	0.00425	-0.0270***	0.0235***	0.00350
	(0.0103)	(0.00828)	(0.00786)	(0.00863)	(0.00746)	(0.00754)
Post*Age 21	0.0134	0.000993	-0.0143	-0.0160	0.0291**	-0.0131
	(0.0152)	(0.0126)	(0.0114)	(0.0184)	(0.0136)	(0.0121)
Constant	0.596***	0.210***	0.194***	0.442***	0.384***	0.174***
	(0.0255)	(0.0187)	(0.0257)	(0.0478)	(0.0305)	(0.0549)
Observations	20010	20010	20010	18599	18599	18599
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change, split by TTWA income in 2010. Income measured median hourly wage, excluding overtime, of each TTWA region (from ASHE). Poorer (richer) TTWAs are the 50% of TTWAs with the lowest (highest) median hourly wage. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, education, ethnicity.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Unemployed search intensity by education - correcting for selection

	(1) Active Low	(2) Active High	(3) >1 m. Low	(4) >1 m. High	(5) # m. Low	(6) # m. High
Post	-0.0526***	-0.0455	0.0131	0.000819	0.00722	0.0683
	(0.0183)	(0.0278)	(0.0107)	(0.0166)	(0.0555)	(0.0933)
Age 21	0.00881	0.0451	0.0244**	-0.0447**	0.101*	-0.197*
	(0.0202)	(0.0355)	(0.0118)	(0.0212)	(0.0615)	(0.119)
Post*Age 21	-0.0328	0.0464	-0.0395**	0.00424	-0.153*	-0.0686
	(0.0281)	(0.0498)	(0.0163)	(0.0297)	(0.0852)	(0.167)
Constant	0.673***	0.581***	0.285***	0.146*	2.327***	2.174***
	(0.0685)	(0.131)	(0.0399)	(0.0783)	(0.209)	(0.441)
Selection eq.						
Post	0.0489	-0.00553	0.0489	-0.00553	0.0500	0.00278
	(0.0319)	(0.0585)	(0.0319)	(0.0585)	(0.0319)	(0.0585)
Age 21	0.0884**	0.206***	0.0884**	0.206***	0.0730**	0.218***
	(0.0351)	(0.0775)	(0.0351)	(0.0775)	(0.0351)	(0.0775)
Post*Age 21	0.108**	-0.0563	0.108**	-0.0563	0.135***	-0.0634
	(0.0492)	(0.109)	(0.0492)	(0.109)	(0.0493)	(0.109)
Constant	0.330***	0.344	0.330***	0.344	0.280**	0.334
	(0.114)	(0.258)	(0.114)	(0.258)	(0.114)	(0.258)
Lambda	-0.113***	-0.0395	-0.0423***	-0.0277	-0.241***	-0.194**
	(0.0225)	(0.0285)	(0.0132)	(0.0170)	(0.0686)	(0.0958)
Observations	15375	4236	15375	4236	15375	4236
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are not in work, split by educational attainment. Low refers to individuals with a maximm of A level / High school education. High refers to individuals with some post-school education. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity. The selection equation includes all controls and treatment variables alongside a variable for studying - the exclusion restriction. Heckman selection model estimated by two-step maximum likelihood.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Unemployed search intensity by regional income - correcting for selection

	(1) Active Poor	(2) Active Rich	(3) >1 m. Poor	(4) >1 m. Rich	(5) # m. Poor	(6) # m. Rich
Post	-0.115***	-0.0450**	0.0153	-0.00462	0.0773	-0.0297
	(0.0320)	(0.0201)	(0.0174)	(0.0121)	(0.0989)	(0.0636)
Age 21	-0.0126	0.0344	0.0000848	0.0101	0.0478	0.0407
	(0.0335)	(0.0211)	(0.0182)	(0.0126)	(0.104)	(0.0667)
Post*Age 21	0.0313	-0.0123	-0.0157	-0.0163	-0.0773	-0.0878
	(0.0498)	(0.0318)	(0.0270)	(0.0191)	(0.154)	(0.101)
Constant	0.709***	0.498***	0.247***	0.167**	2.345***	1.857***
	(0.106)	(0.112)	(0.0577)	(0.0671)	(0.334)	(0.353)
Selection eq.						
Post	0.0189	0.0100	0.0189	0.0100	0.0270	0.0120
	(0.0576)	(0.0368)	(0.0576)	(0.0368)	(0.0577)	(0.0368)
Age 21	0.150**	0.0715*	0.150**	0.0715*	0.123**	0.0653*
	(0.0610)	(0.0385)	(0.0610)	(0.0385)	(0.0611)	(0.0386)
Post*Age 21	-0.0381	0.0824	-0.0381	0.0824	-0.000157	0.0963*
	(0.0916)	(0.0584)	(0.0916)	(0.0584)	(0.0916)	(0.0584)
Constant	0.309*	0.460**	0.309*	0.460**	0.241	0.472**
	(0.176)	(0.202)	(0.176)	(0.202)	(0.176)	(0.202)
Lambda	-0.188***	-0.0446**	-0.0541**	-0.0383***	-0.276**	-0.245***
	(0.0446)	(0.0208)	(0.0246)	(0.0124)	(0.140)	(0.0656)
Observations	4329	12350	4329	12350	4329	12350
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are not in work, split by average income of their resident TTWA. Poor refers to the poorest half of TTWAs, rich to the richest half of TTWAs. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity. The selection equation includes all controls and treatment variables alongside a variable for studying - the exclusion restriction. Heckman selection model estimated by two-step maximum likelihood.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Unemployed search duration by education - correcting for selection

	(1)	(2)	(3)	(4)
Outcome:	TimeA	TimeA	TimeB	TimeB
Education	Low	High	Low	High
Post	2.153***	0.249	1.737*	-0.105
	(0.813)	(0.298)	(0.957)	(0.485)
Age 21	-1.220*	-0.830*	-1.629**	-0.415
	(0.688)	(0.478)	(0.767)	(0.669)
Post*Age 21	1.589	1.138	2.368*	0.427
	(1.099)	(0.743)	(1.248)	(0.931)
Constant	22.05***	10.21***	19.15***	9.840***
	(2.198)	(1.487)	(2.298)	(1.392)
Selection eq.				
Post	0.0306	-0.0185	0.186***	0.388***
	(0.0392)	(0.0768)	(0.0360)	(0.102)
Age 21	0.0411	0.118	0.0670	-0.0237
	(0.0463)	(0.0810)	(0.0478)	(0.0870)
Post*Age 21	0.145**	0.0173	0.167**	0.333**
	(0.0595)	(0.130)	(0.0652)	(0.150)
Constant	0.434***	0.0319	0.511***	0.449
	(0.111)	(0.346)	(0.117)	(0.316)
ρ	-0.0870***	0.00446	-0.0507	-0.0711**
	(0.0280)	(0.0628)	(0.0394)	(0.0334)
$\ln(\sigma)$	2.677***	1.791***	2.683***	1.774***
	(0.0253)	(0.0540)	(0.0256)	(0.0689)
Observations	15375	4236	15375	4236
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity and occupation. TimeA refers to self-reported unemployment searching duration. TimeB is a separate self-reported measure of time spent job-seeking.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Unemployed search duration by regional income - correcting for selection

	(1)	(2)	(3)	(4)
Outcome:	TimeA	TimeA	TimeB	TimeB
Regional income:	Poor	Rich	Poor	Rich
Post	1.235	1.514*	0.487	1.659
	(0.869)	(0.789)	(1.046)	(1.056)
Age 21	-1.234	-1.268*	-2.002**	-1.133
	(0.776)	(0.763)	(0.867)	(0.950)
Post*Age 21	2.153	2.031**	3.857**	2.010
	(1.397)	(1.028)	(1.533)	(1.261)
Constant	15.75***	16.66***	13.26***	16.45***
	(2.091)	(2.296)	(2.524)	(3.094)
Selection eq.				
Post	0.000589	0.0333	0.210***	0.250***
	(0.0495)	(0.0435)	(0.0478)	(0.0452)
Age 21	0.0712	0.0511	0.0630	0.0688
	(0.0533)	(0.0540)	(0.0590)	(0.0511)
Post*Age 21	0.112	0.132**	0.140*	0.182**
	(0.0768)	(0.0652)	(0.0805)	(0.0825)
Constant	0.324**	0.309**	0.523***	0.290**
	(0.151)	(0.130)	(0.142)	(0.131)
ρ	-0.0936***	-0.0386	-0.0713***	0.00106
	(0.0219)	(0.0324)	(0.0171)	(0.0597)
$ln(\sigma)$	2.602***	2.504***	2.627***	2.538***
	(0.0334)	(0.0389)	(0.0350)	(0.0386)
Observations	9048	9689	9048	9689
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity and occupation. TimeA refers to self-reported unemployment searching duration. TimeB is a separate self-reported measure of time spent job-seeking. Model is a Heckman Selection model, estimated by maximum likelihood.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.5 Additional results for on-the-job search

Table A.11: On-the-job search estimates by education

	Any OJS		Replacement job only	
	(1)	(2)	(3)	(4)
	Low	High	Low	High
Post	0.0182***	0.0298**	0.0182***	0.0289**
	(0.00648)	(0.0132)	(0.00539)	(0.0118)
Age 21	0.00409	-0.0413**	0.00114	-0.0468***
	(0.00790)	(0.0180)	(0.00704)	(0.0164)
Post*Age 21	0.00605	0.00570	0.00475	0.00462
	(0.0130)	(0.0292)	(0.0115)	(0.0256)
Constant	0.148	0.458**	0.158*	0.484***
	(0.0914)	(0.184)	(0.0916)	(0.184)
Observations	23556	9476	23541	9453
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity and occupation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: On-the-job search estimates by regional income - NUTS2 regions

	Any OJS		Replacement job only	
	(1)	(2)	(3)	(4)
	Poor	Rich	Poor	Rich
Post	0.0258***	0.0136	0.0235***	0.0159**
	(0.00836)	(0.00852)	(0.00689)	(0.00658)
Age 21	0.00346	-0.0175	-0.00458	-0.0156
	(0.0106)	(0.0122)	(0.00961)	(0.00986)
Post*Age 21	0.00493	0.0152	0.00231	0.0135
	(0.0178)	(0.0179)	(0.0160)	(0.0141)
Constant	0.149	0.280**	0.172	0.273**
	(0.133)	(0.127)	(0.132)	(0.128)
Observations	15777	15650	15756	15635
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity, education and occupation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: On-the-job search estimates by regional income - TTWA regions

	OJS		Replace	
	(1)	(2)	(3)	(4)
	Poor	Rich	Poor	Rich
Post	0.0199**	0.0154*	0.0133	0.0219***
	(0.00906)	(0.00911)	(0.00823)	(0.00820)
Age 21	0.00824	-0.0200*	-0.00169	-0.0158
	(0.00949)	(0.0118)	(0.00846)	(0.00973)
Post*Age 21	0.00601	0.0213	0.00666	0.0126
	(0.0184)	(0.0189)	(0.0174)	(0.0151)
Constant	0.108	0.332**	0.133	0.344***
	(0.133)	(0.129)	(0.133)	(0.129)
Observations	13777	14490	13757	14479
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 21-23 year olds, 24 months either side of 2010 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, ethnicity, education and occupation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: On-the-job search intensity estimates by education - correcting for selection

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Active	Active	>1 m.	>1 m.	# m.	# m.
Education:	Low	High	Low	High	Low	High
Post	0.130	-0.0261	0.412	0.0851	1.247	0.0339
	(0.606)	(0.119)	(1.375)	(0.146)	(4.348)	(0.282)
Age 21	0.0320	0.0682	0.0936	-0.0978	0.296	-0.184
	(0.143)	(0.143)	(0.325)	(0.177)	(1.029)	(0.340)
Post*Age 21	0.0602	-0.0376	0.0926	-0.0102	0.385	0.0484
	(0.170)	(0.0522)	(0.386)	(0.0863)	(1.220)	(0.141)
Constant	-2.556	0.119	-6.230	-1.195	-18.76	-0.376
	(9.468)	(1.718)	(21.50)	(2.076)	(67.99)	(4.049)
Selection eq.						
Post	0.105***	0.118***	0.105***	0.118***	0.105***	0.118***
	(0.0285)	(0.0354)	(0.0285)	(0.0354)	(0.0285)	(0.0354)
Age 21	0.0206	-0.135**	0.0206	-0.135**	0.0206	-0.135**
	(0.0326)	(0.0554)	(0.0326)	(0.0554)	(0.0326)	(0.0554)
Post*Age 21	0.0238	0.000176	0.0238	0.000176	0.0238	0.000176
	(0.0460)	(0.0782)	(0.0460)	(0.0782)	(0.0460)	(0.0782)
Constant	-0.774***	-0.659*	-0.774***	-0.659*	-0.774***	-0.659*
	(0.264)	(0.360)	(0.264)	(0.360)	(0.264)	(0.360)
Lambda	2.114	0.174	4.800	1.027	15.18	1.259
	(6.969)	(1.321)	(15.82)	(1.579)	(50.04)	(3.101)
Observations	23593	9482	23593	9482	23593	9482
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are in work. The selection equation includes all controls and treatment variables. Heckman selection model estimated by two-step maximum likelihood. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: On-the-job search intensity estimates by regional income - correcting for selection

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:	Active	Active	>1 m.	>1 m.	# m.	# m.
Region:	Poor	Rich	Poor	Rich	Poor	Rich
Post	-0.0462	0.0444	0.00566	-0.0273	-0.108	0.150
	(0.0629)	(0.143)	(0.0402)	(0.0645)	(0.175)	(0.347)
Age 21	0.0122	-0.0160	-0.0228	0.0120	-0.0601	-0.163
	(0.0631)	(0.0876)	(0.0408)	(0.0381)	(0.176)	(0.213)
Post*Age 21	-0.0248	0.0832	0.00532	0.00484	0.111	0.313
	(0.0858)	(0.109)	(0.0563)	(0.0460)	(0.241)	(0.265)
Constant	1.227	-1.143	0.831	0.459	4.109	-2.042
	(2.037)	(2.186)	(1.234)	(1.004)	(5.586)	(5.323)
Selection eq.						
Post	0.0464	0.108***	0.0464	0.108***	0.0464	0.108***
	(0.0476)	(0.0286)	(0.0476)	(0.0286)	(0.0476)	(0.0286)
Age 21	0.0428	-0.0606*	0.0428	-0.0606*	0.0428	-0.0606*
	(0.0527)	(0.0338)	(0.0527)	(0.0338)	(0.0527)	(0.0338)
Post*Age 21	0.0504	0.0699	0.0504	0.0699	0.0504	0.0699
	(0.0806)	(0.0503)	(0.0806)	(0.0503)	(0.0806)	(0.0503)
Constant	-0.927	-0.797***	-0.927	-0.797***	-0.927	-0.797***
	(0.626)	(0.261)	(0.626)	(0.261)	(0.626)	(0.261)
Lambda	-0.497	1.130	-0.472	-0.124	-1.616	2.752
	(1.345)	(1.580)	(0.807)	(0.730)	(3.680)	(3.846)
Observations	8163	20157	8163	20157	8163	20157
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. Sample is 21-23 year olds, 24 months either side of 2010 policy change who are in work. The selection equation includes all controls and treatment variables. Heckman selection model estimated by two-step maximum likelihood. Controls included: sex, region (NUTS2 level), quarter, proxy, education, marital status, ethnicity.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.6 2016 new age tier

Table A.16: Unemployed search extensive margin estimates - 2016 new age tier

	(1) Working	(2) Unemployed	(3) Inactive	(4) Student
Post	0.00816	-0.00823*	0.000692	-0.000623
	(0.00813)	(0.00491)	(0.00526)	(0.00471)
Age 25+	0.0671***	-0.0200***	0.00923**	-0.0563***
	(0.00769)	(0.00395)	(0.00457)	(0.00470)
Post*Age 25+	0.00140	-0.0000717	-0.00416	0.00284
	(0.00936)	(0.00564)	(0.00657)	(0.00510)
Constant	0.410***	0.0630***	0.507***	0.0199
	(0.0827)	(0.0178)	(0.0765)	(0.0233)
Observations	47651	47651	47651	47651
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 22-28 year olds, 12 months before and 9 months after 2016 policy change. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, education, ethnicity.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.17: Unemployed search extensive margin estimates, excluding students - 2016 new age tier

	(1) Working	(2) Unemployed	(3) Inactive
Post	0.00762	-0.00883*	0.00121
	(0.00761)	(0.00533)	(0.00570)
Age 25+	0.0216***	-0.0257***	0.00410
	(0.00626)	(0.00433)	(0.00488)
Post*Age 25+	0.00369	0.000496	-0.00419
	(0.00875)	(0.00605)	(0.00695)
Constant	0.424***	0.0655***	0.511***
	(0.0801)	(0.0196)	(0.0772)
Observations	45096	45096	45096
Controls	Yes	Yes	Yes

Standard errors in parentheses, clustered on age-region level. Sample is 22-28 year olds, 12 months before and 9 months after 2016 policy change, not excluded from labour force due to studying. Controls included: sex, region (NUTS2 level), quarter, proxy, marital status, education, ethnicity.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Appendix for Chapter 2

B.1 Descriptive statistics

Table B.1 displays basic descriptive statistics for the Business Structure Database firm (plant) level dataset used. Key variables displayed include the plant level employment, number of masslayoffs within 20km of the plant, distance to the closest mass layoff in the year in question (if there exists a mass layoff within 50km), the number of firms not located within 50km of a mass layoff, and the employment change at the firm level.

Table B.1: Descriptive statistics: firms (plants)

Firm Variable	Mean	Std. Dev.	p25	p50	p75	N
Employment	7.679	83.243	1	2	5	71,128,244
# ML within 20km	5.136	11.2371	0	1	4	71,128,248
Dist to closest ML if <50km	15.689	13.134	4.7	11.9	24.2	58,755,900
No ML within 50km	-	-	-	-	-	12,372,348
Employment change	-.699	41.479	0	0	0	67,482,554

ML refers to mass layoff in the year in question. Sample is 1997-2017 of the BSD

Table B.2 displays descriptive statistics for the sample of mass layoffs. On average, there are 100 mass layoffs of more than 1,000 workers per year over the sampled period. These are spread across a range of industries, as displayed by the 1 digit industry percentage counts.

Table B.2: Descriptive statistics: masslayoffs

Masslayoff Variable	Mean	Std. Dev.	p25	p50	p75	N
Employment change	-2732	5567	-1200	-1500	-2300	2,009

1 digit industry	Count	Percent
0-1	72	3.58%
2	140	6.97%
3	104	5.18 %
4	123	6.12%
5	305	15.18%
6	496	24.69%
7	662	32.95%
8	21	1.05%
9	86	4.28%

B.2 Robustness checks

I build up the analysis using a variety of fixed effects, eliminating sources of variation one-by-one to assuage some concerns about endogeneity. The baseline formulation uses 2 digit SIC industry, 2 digit postcode and time fixed effects. This removes industry trends, location fixed effects and non-parametric time-specific shocks shared by all firms. The variation driving the results remains proximity to masslayoffs, absent all these fixed effects.

Concern remains that individual industries or regions may have time varying shocks, correlated with proximity, that are driving these results. I therefore add industry-year interactions, to control for national industry shocks in each year. This would remove endogeneity stemming from masslayoff and non-masslayoff firms spatially sorting close together based on industry. The employment loss observed would be from correlated shocks, rather than true proximity spillovers. The results remain robust to this. The apparent lack of concern with industrial spatial sorting is consistent with the industrial closeness results of section 2.6.1 - all industries, whether close or not, appear to be affected by spatial spillovers.

Furthermore, I add location-year interactions. However, as 2 digit postcode locations cover a several kilometer radius these remove a lot of the year-to-year variation I am interested in. The results, unsurprisingly, are therefore substantially weakened.

Next, I check for robustness around other mass layoffs. Table B.3 displays several robustness checks. Column 1 controls for serial correlation in the mass layoff treatment variable, by controlling for the distance to the closest mass layoff in the previous year. For simplicity of display, the parametric form $\exp(-dist)$ is used for the lagged mass layoff, which is approximately one for very close firms and approximately zero for far away firms. The negative coefficient implies negative employment spillovers that decay with distance - as expected. Importantly, the spillover distance estimates for the current year's closest mass layoff are not significantly altered.

Columns 2 and 3 control for other mass layoffs in the same year. Column two controls for the distance to the second closest mass layoff, again using the parametric form $\exp(-dist)$. A negative coefficient implies negative but decaying employment spillovers from the second closest, but the estimated impact of the first closest is only very slightly weakened. Column three controls for the number of mass layoffs within 20km - checking for the possibility that the estimates are contaminated by spatial clustering of mass layoffs. The employment spillover distance decay estimates are unaffected.

Lastly, column four displays a standard placebo check - the analogue of the parallel trends assumption used in difference-in-differences approaches. The distance dummies are replaced by the distance to the closest mass layoff one period in the future. Significant results would call into question the event study approach - either by demonstrating anticipation effects or indicating that the shocks may not be as exogenous as hoped. We see no significant estimates, assuaging endogeneity concerns.

There may also be concern that the Global Financial Crisis (GFC) was the driving force behind the results. The sampled period of 1997-2017 includes the timeframe, and the pooled results may simply be an average of large GFC effects and zero effects at other times. Table B.4 segments the sample into different time periods and runs separate regressions on each. We see that negative employment spillovers in close proximity to mass layoffs were somewhat stronger in the GFC year (approximately 2007-2012) but present and significant during all time periods.

Lastly, I also consider a variety of alternative corrections for non-spherical errors. Results remain significant in all instances. Table B.5 displays some of the forms: clustered errors at the two digit industry level (allowing for national shocks correlated across related industries), at the regional level (allowing for shocks correlated within regions) and at the industry-regional interaction level.

Table B.3: Controlling for other mass layoffs in the spatial distribution of effects on impact

	(1)	(2)	(3)	(4)
Extra controls:	ML Lag 1	2nd closest ML	# ML in 20km	Placebo
Dep. var.	$\Delta \log(L)$	$\Delta \log(L)$	$\Delta \log(L)$	$\Delta \log(L)$
dist 0-1km	-0.0696***	-0.0521***	-0.0716***	0.00127
	(0.00769)	(0.00615)	(0.00739)	(0.000981)
dist 1-2km	-0.0260***	-0.0237***	-0.0270***	0.000728
	(0.00357)	(0.00338)	(0.00354)	(0.000665)
dist 2-3km	-0.0140***	-0.0154***	-0.0152***	0.000256
	(0.00298)	(0.00304)	(0.00296)	(0.000369)
dist 3-4km	-0.00740**	-0.00984***	-0.00860**	0.000473
	(0.00263)	(0.00270)	(0.00264)	(0.000518)
dist 4-5km	-0.00718**	-0.00985***	-0.00787**	-0.0000525
	(0.00250)	(0.00256)	(0.00267)	(0.000398)
dist 5-10km	-0.00606***	-0.00839***	-0.00630**	-0.0000811
	(0.00171)	(0.00180)	(0.00201)	(0.000401)
dist 10-20km	-0.00349**	-0.00490***	-0.00351*	-0.000233
	(0.00117)	(0.00118)	(0.00145)	(0.000376)
dist 20-40km	0.000539	-0.000310	0.000589	-0.000539*
	(0.00120)	(0.00119)	(0.00136)	(0.000241)
exp(-dist) lagged	-0.00594***			
	(0.00122)			
exp(-dist) 2nd closest		-0.0999***		
		(0.0162)		
# ML in 20km			0.000385*	
			(0.000170)	
Constant	-0.0410***	-0.0425***	-0.0439***	-4.155***
	(0.00659)	(0.00643)	(0.00645)	(0.405)
Controls	Yes	Yes	Yes	Yes
Observations	66,014,522	66,014,522	66,014,522	54,990,618
R^2	0.023	0.023	0.023	0.001

Standard errors in parentheses, clustered at 2 digit industry level. Controls are 2 digit SIC industry, 2 digit postcode, and year, plus the additional mass layoff controls displayed. Column 1 controls for the distance to the closest mass layoff in the previous year, column 2 for the distance to the second closest in the current year and column 3 for the number of mass layoffs within 20km in the current year. Column 4 is a placebo regression, where the distance dummies are the distance to the closest mass layoff one year in the future. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.4: Global Financial Crisis segmentation

	(1)	(2)	(3)	(4)
Years:	< 2007	≥ 2007	2007-2012	2013-2017
Dep. var.	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$
dist 0-1km	-0.0585***	-0.0795***	-0.0905***	-0.0564***
	(0.00455)	(0.0109)	(0.0140)	(0.00606)
dist 1-2km	-0.0251***	-0.0284***	-0.0227***	-0.0329***
	(0.00275)	(0.00460)	(0.00654)	(0.00313)
dist 2-3km	-0.0134***	-0.0163***	-0.0178**	-0.0128***
	(0.00290)	(0.00380)	(0.00587)	(0.00201)
dist 3-4km	-0.0117***	-0.00584	-0.00736	-0.00133
	(0.00273)	(0.00331)	(0.00542)	(0.00157)
dist 4-5km	-0.0110***	-0.00582	-0.00576	-0.00328
	(0.00277)	(0.00308)	(0.00499)	(0.00177)
dist 5-10km	-0.0102***	-0.00461*	-0.00549	-0.000438
	(0.00256)	(0.00201)	(0.00320)	(0.00155)
dist 10-20km	-0.00753***	-0.00167	-0.000474	-0.00183
	(0.00217)	(0.00135)	(0.00191)	(0.00194)
dist 20-40km	-0.00335*	0.00269	0.00523	-0.000128
	(0.00139)	(0.00208)	(0.00302)	(0.00150)
Constant	-0.0503***	0.0422***	0.0488***	-0.0387***
	(0.00624)	(0.00901)	(0.00950)	(0.00465)
Controls	Yes	Yes	Yes	Yes
Observations	27,759,558	38,254,964	21,671,828	16,583,136
R^2	0.017	0.028	0.040	0.010

Standard errors in parentheses, clustered at 2 digit industry level. Controls are 2 digit SIC industry, 2 digit postcode, and year. Full sample is 1997-2017, subsamples indicated in column headers.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.5: Error clustering: industry, region, industry-region two way

	(1)	(2)	(3)
Clustering:	Industry	Region	Ind-Reg
Dep. var:	$\Delta \log(L)$	$\Delta \log(L)$	$\Delta \log(L)$
dist 0-1km	-0.0702***	-0.0702***	-0.0702***
	(0.00769)	(0.00559)	(0.00316)
dist 1-2km	-0.0266***	-0.0266***	-0.0266***
	(0.00362)	(0.00300)	(0.00201)
dist 2-3km	-0.0147***	-0.0147***	-0.0147***
	(0.00305)	(0.00269)	(0.00182)
dist 3-4km	-0.00813**	-0.00813**	-0.00813***
	(0.00269)	(0.00278)	(0.00177)
dist 4-5km	-0.00792**	-0.00792**	-0.00792***
	(0.00254)	(0.00264)	(0.00181)
dist 5-10km	-0.00683***	-0.00683**	-0.00683***
	(0.00179)	(0.00261)	(0.00160)
dist 10-20km	-0.00424***	-0.00424	-0.00424**
	(0.00119)	(0.00264)	(0.00142)
dist 20-40km	-0.0000507	-0.0000507	-0.0000507
	(0.00121)	(0.00187)	(0.00129)
Constant	-0.0426***	-0.0426***	-0.0426***
	(0.00641)	(0.00912)	(0.00904)
Controls	Yes	Yes	Yes
Observations	66,014,522	66,014,522	66,014,522
R^2	0.023	0.023	0.023

Standard errors in parentheses. Column 1 clusters the errors at 2 digit industry level, column 2 at the 2 digit region level (the letters only for UK postcodes - effectively a small-medium sized city + hinterland), and column three at the 2 digit industry-region interaction level. Controls are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B.3 Additional results for section 2.3: spatial results on impact

B.3.1 Comparing baseline results to parametric functions

Much of the spatial literature (e.g. Ahlfeldt et al. (2015)) use exponential decay functions to capture the effects of distance. Figure B.1 overlays two candidate exponential decay functions to the graphed non-parametric estimates. As can be observed, a single exponential decay function cannot accurately capture the spatial spillovers: the effects decay far more rapidly at short distances than they do at further distances. The rapid decay patterns, up to about 4km in distance, can be roughly captured by the red line which plots a scale parameter of 10 and a decay parameter of -0.75. After about 5km from the masslayoff, the decay rate is much less rapid, albeit the magnitude of the effects are much smaller. A scale parameter of 1.5 and a much slower decay parameter of -0.1 provide a better fit.

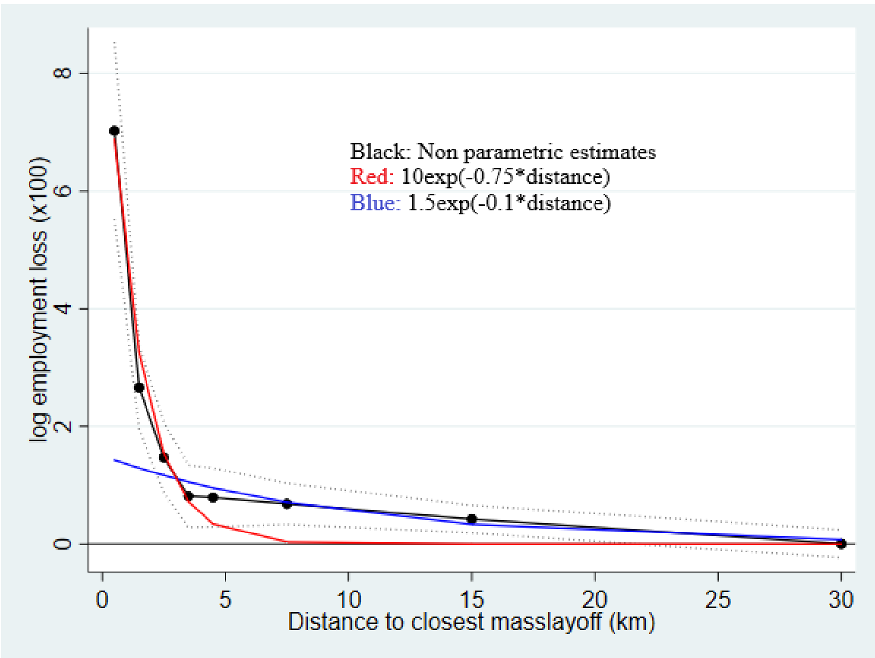

Figure B.1: Exponential decay functions versus non parametric estimates. The black line plots the column three non-parametric estimates from Table 2.1, including 95% confidence intervals. The red line overlays an exponential decay function of the form $10\exp(-0.75 * dist)$. The blue line overlays a second exponential decay function of the form $1.5\exp(-0.1 * dist)$

B.3.2 Firm type heterogeneity tables

Table B.6: Partitioning the sample based on firm size (employment count)

	(1)	(2)	(3)	(4)	(5)
Firm size:	< 10	< 20	> 50	> 100	20-50
Dep. Var	$\Delta \log(L)$	$\Delta \log(L)$	$\Delta \log(L)$	$\Delta \log(L)$	$\Delta \log(L)$
dist 0-1km	-0.0229***	-0.0391***	-0.134***	-0.151**	-0.0902***
	(0.00461)	(0.00576)	(0.0383)	(0.0480)	(0.0233)
dist 1-2km	-0.00734**	-0.0139***	-0.0654***	-0.0812**	-0.0321***
	(0.00220)	(0.00276)	(0.0166)	(0.0255)	(0.00800)
dist 2-3km	-0.00496*	-0.00844***	-0.0261*	-0.0354	-0.0186*
	(0.00201)	(0.00242)	(0.0127)	(0.0186)	(0.00840)
dist 3-4km	-0.00323	-0.00500*	-0.00391	-0.0102	-0.00248
	(0.00203)	(0.00234)	(0.0121)	(0.0178)	(0.00599)
dist 4-5km	-0.00352	-0.00469*	-0.0102	-0.0267	-0.0131**
	(0.00199)	(0.00229)	(0.00788)	(0.0134)	(0.00464)
dist 5-10km	-0.00354*	-0.00454**	-0.00416	-0.0166	-0.00615
	(0.00145)	(0.00162)	(0.00619)	(0.00837)	(0.00680)
dist 10-20km	-0.00284**	-0.00323**	-0.00335	-0.0106	-0.00473
	(0.00102)	(0.00111)	(0.00564)	(0.00806)	(0.00395)
dist 20-40km	-0.000455	-0.000168	0.00484	0.000681	0.00206
	(0.00112)	(0.00125)	(0.00401)	(0.00609)	(0.00351)
Constant	-0.0242***	-0.0320***	-0.606***	-0.735***	-0.376***
	(0.00430)	(0.00500)	(0.0302)	(0.0438)	(0.0137)
Controls	Yes	Yes	Yes	Yes	Yes
N	57,353,827	61,900,460	1,403,569	613,127	2,710,493
R^2	0.023	0.024	0.048	0.050	0.043

Standard errors in parentheses, clustered at 2 digit industry level. Controls are 2 digit SIC industry, 2 digit postcode, and year fixed effects. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.7: Partitioning sample based on whether firm or closest mass layoff was in the manufacturing industry

	(1)	(2)	(3)	(4)
Type:	Man ML	Non-man ML	Man firm	Non-man firm
Dep. var	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$
dist 0-1km	-0.0508***	-0.0721***	-0.120***	-0.0672***
	(0.0129)	(0.00733)	(0.0154)	(0.00740)
dist 1-2km	-0.0168*	-0.0275***	-0.0300***	-0.0261***
	(0.00767)	(0.00343)	(0.00419)	(0.00376)
dist 2-3km	-0.0124	-0.0147***	-0.0235***	-0.0140***
	(0.00740)	(0.00287)	(0.00358)	(0.00312)
dist 3-4km	-0.00844	-0.00793**	-0.0117***	-0.00776**
	(0.00788)	(0.00245)	(0.00205)	(0.00279)
dist 4-5km	-0.00633	-0.00758***	-0.0131***	-0.00737**
	(0.0105)	(0.00217)	(0.00229)	(0.00260)
dist 5-10km	-0.00687	-0.00642***	-0.0103***	-0.00645**
	(0.00660)	(0.00178)	(0.00266)	(0.00185)
dist 10-20km	-0.000360	-0.00422**	-0.00508*	-0.00413**
	(0.00465)	(0.00143)	(0.00187)	(0.00127)
dist 20-40km	0.00410	0.0000103	0.00225	-0.000347
	(0.00356)	(0.00148)	(0.00153)	(0.00131)
Constant	-0.0548***	-0.0421***	-0.380***	-0.0393***
	(0.00902)	(0.00668)	(0.0164)	(0.00625)
Controls	Yes	Yes	Yes	Yes
N	10,072,080	55,909,683	4,125,366	61,889,156
R^2	0.029	0.022	0.026	0.023

Standard errors in parentheses, clustered at 2 digit industry level. Controls are 2 digit SIC industry, 2 digit postcode, and year fixed effects. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.8: Employment density around the firm

	(1) $\Delta log(L)$ Impact	(2) $\Delta log(L)$ Lag 1	(3) $\Delta log(L)$ Impact	(4) $\Delta log(L)$ Lag 1
exp(-dist)	-0.101***	-0.0717***	-0.112***	-0.0847***
	(0.000606)	(0.000607)	(0.000627)	(0.000635)
Dens	-0.00664***	-0.00909***	-0.0158***	-0.0173***
	(0.000112)	(0.000111)	(0.000174)	(0.000162)
exp(-dist) * Dens			0.0208***	0.0210***
			(0.000300)	(0.000300)
Cons	-0.0464***	-0.0492***	-0.0486***	-0.0514***
	(0.000813)	(0.000813)	(0.000813)	(0.000813)
FEs	Yes	Yes	Yes	Yes
N	66,016,397	66,016,397	66,016,397	66,016,397
R^2	0.023	0.023	0.023	0.023

Standard errors in parentheses, clustered on 2 digit industry. Exp(-dist) is the exponential of the negative distance to the closest masslayoff. Dens is the standardised employment per square km in a 3by3km grid around the plant. Controls are 2 digit SIC industry, 2 digit postcode, and year fixed effects. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.9: Partitioning the firm sample based on employment density

	(1)	(2)	(3)	(4)
Emp. density	Top 50%	Bottom 50%	Top 25%	Bottom 25%
Dep. var	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$
dist 0-1km	-0.0515***	-0.0420***	-0.0465***	-0.0271**
	(0.00645)	(0.00529)	(0.00671)	(0.00980)
dist 1-2km	-0.0113***	-0.0102**	-0.00834	-0.00482
	(0.00298)	(0.00324)	(0.00429)	(0.00537)
dist 2-3km	-0.00162	-0.00701*	-0.000987	-0.00829*
	(0.00234)	(0.00311)	(0.00387)	(0.00352)
dist 3-4km	0.00372*	-0.00588*	0.00451	-0.00270
	(0.00175)	(0.00285)	(0.00310)	(0.00275)
dist 4-5km	0.00134	-0.00500	-0.00104	-0.00472
	(0.00200)	(0.00276)	(0.00381)	(0.00321)
dist 5-10km	-0.00305	-0.00438	-0.00932**	-0.00438
	(0.00160)	(0.00231)	(0.00295)	(0.00249)
dist 10-20km	-0.00666***	-0.00298*	-0.0140***	-0.00321*
	(0.00143)	(0.00141)	(0.00284)	(0.00153)
dist 20-40km	-0.000199	-0.000912	0.00291	-0.00115
	(0.00280)	(0.000601)	(0.00553)	(0.000832)
Constant	-0.0834***	-0.0320***	-0.0971***	-0.0282***
	(0.00974)	(0.00489)	(0.0100)	(0.00466)
Controls	Yes	Yes	Yes	Yes
N	32,794,533	33,219,989	16,267,241	16,665,832
R^2	0.024	0.021	0.026	0.021

Standard errors in parentheses, clustered on 2 digit industry. Sample is segmented based on the standardised employment per square kilometer in a 3km-by-3km grid around the plant. Controls are 2 digit SIC industry, 2 digit postcode, and year fixed effects. Years included are 1997-2017.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B.4 Dynamic results table

Table B.10: Estimates for the dynamic impacts of mass layoffs: up to five time periods after the event

	(1) T1 $\Delta \log(L)$	(2) T2 $\Delta \log(L)$	(3) T3 $\Delta \log(L)$	(4) T4 $\Delta \log(L)$	(5) T5 $\Delta \log(L)$
dist 0-1km	-0.0590***	-0.0595***	-0.0586***	-0.0586***	-0.0480***
	(0.00510)	(0.00520)	(0.00270)	(0.00270)	(0.00326)
dist 1-2km	-0.0254***	-0.0286***	-0.0267***	-0.0267***	-0.0195***
	(0.00321)	(0.00197)	(0.00186)	(0.00186)	(0.00173)
dist 2-3km	-0.0137***	-0.0158***	-0.0179***	-0.0179***	-0.0122***
	(0.00236)	(0.00173)	(0.00143)	(0.00143)	(0.00188)
dist 3-4km	-0.00866***	-0.00694***	-0.0127***	-0.0127***	-0.00566***
	(0.00171)	(0.00168)	(0.00123)	(0.00123)	(0.00159)
dist 4-5km	-0.00878***	-0.00723***	-0.0109***	-0.0109***	-0.00344
	(0.00165)	(0.00176)	(0.00137)	(0.00137)	(0.00197)
dist 5-10km	-0.00802***	-0.00742***	-0.00834***	-0.00834***	-0.00340
	(0.00161)	(0.00164)	(0.00111)	(0.00111)	(0.00206)
dist 10-20km	-0.00654***	-0.00565***	-0.00574***	-0.00574***	-0.00173
	(0.00124)	(0.00130)	(0.00117)	(0.00117)	(0.00204)
dist 20-40km	-0.00372***	-0.00355**	-0.00186	-0.00186	-0.000112
	(0.000938)	(0.00128)	(0.00102)	(0.00102)	(0.00164)
Constant	-0.0447***	-0.0561***	-0.0641***	-0.0641***	-0.0454**
	(0.00681)	(0.00763)	(0.00750)	(0.00750)	(0.0155)
Controls	Yes	Yes	Yes	Yes	Yes
N	66,014,522	63,245,479	60,450,337	60,450,337	54,824,033
R^2	0.023	0.023	0.024	0.024	0.026

Years included are 1998-2017 for T1, 1999-2017 for T2, 2000-2017 for T3, 2001-2017 for T4, 2002-2017 for T5. Standard errors in parentheses, clustered on 2 digit industry. Variables are a set of dummies indicating distance to closest masslayoff. Fixed effects are 2 digit SIC industry, 2 digit postcode, and year. The estimates are plotted in figure 2.5 in section 2.5.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B.5 Firm churn and aggregate local impacts

The analysis so far estimates the employment response of existing firms to a nearby masslayoff. We see existing firms in close proximity to a mass layoff reduce their employment substantially. However, a related margin of adjustment is the impact on firm churn. Firm churn may be a large part of local adjustment to masslayoffs - some firms may shut down while new firms may establish in their place. Another way of phrasing this is that firm dynamism may rise in response to masslayoffs. Here, I estimate the direct impact on firm birth and death of mass layoff proximity.[1]

Table B.11 displays estimates of the impact on firm birth and death. Columns 1 and 2 are linear probability models where the dependent variable equals one if the life event (birth or death respectively) occurred and zero if it did not (i.e. the firm existed in both $t-1$ and t). We see that both birth rates and death rates are higher in close proximity to a mass layoff. However, the increase in death rates is relatively larger - approximately four times the increase in birth rates for firms located within 1 kilometre of the mass layoff. In short, churn increases, but the increase in firm death outweighs firm birth.

Column 3 extends the baseline analysis by including firms born implicitly in the analysis. The dependent variable, $\Delta \log L_t = (\log L_t - \log L_{t-1})$ is set to $(\log L_t)$ for those firms born between $t-1$ and t. In effect, this sets their lagged employment to 1 to avoid the $\log 0$ issue. We see that the overall firm level employment effect close to mass layoffs is still negative, but weakened once firm birth is included in the picture.

The baseline analysis estimates firm level adjustment, not regional or aggregate level adjustment. Regional or aggregate estimates would require aggregation, and therefore the firm level estimates to be weighted by firm size. Table B.12 therefore weights the firm level changes in a variety of ways. Column 1 is the baseline, unweighted estimates, column 2 weights the baseline estimates by firm employment levels, and columns 3 and 4 weight the firms by their log employment. Columns 1-3 are the baseline samples, including only those firms alive at the start of the period. Column 4 adds in firms born during the time period. The weighted regressions have more negative point estimates than their unweighted counterparts. This is consistent with earlier estimates that larger firms respond more strongly.

[1]Existing firms that die during the time period in question are included implicitly in the baseline analysis. Those firms have their $\Delta \log L_t = (\log L_t - \log L_{t-1})$ set to $(-\log L_{t-1})$, i.e. $\log L_t = 0$. In effect, this assumes that all but one of their employees leave the firm, so as to avoid the $\log 0$ issue. However, the direct effect on firm death is not estimated, while firm birth has been entirely abstracted from so far.

Table B.11: Firm churn: the impact on firm birth and death

	(1)	(2)	(3)
Dep var:	Birth	Death	$\Delta\log(L)$
dist 0-1km	0.00527***	0.0200***	-0.0243***
	(0.00119)	(0.00411)	(0.00589)
dist 1-2km	0.00682***	0.00748**	-0.00310
	(0.00163)	(0.00220)	(0.00272)
dist 2-3km	0.00489**	0.00287	-0.00370
	(0.00143)	(0.00191)	(0.00227)
dist 3-4km	0.00455**	0.00129	-0.000548
	(0.00135)	(0.00178)	(0.00166)
dist 4-5km	0.00314*	0.00194	-0.00184
	(0.00122)	(0.00149)	(0.00135)
dist 5-10km	0.00250**	0.000380	-0.00173
	(0.000907)	(0.00140)	(0.001000)
dist 10-20km	0.00336	-0.00150	-0.000901
	(0.00250)	(0.00107)	(0.000912)
dist 20-40km	0.00107	0.000373	0.00109
	(0.00101)	(0.00126)	(0.000768)
Constant	0.0742***	0.0627***	-0.0102
	(0.00865)	(0.00429)	(0.00600)
Controls	Yes	Yes	Yes
Observations	76,505,519	76,505,519	74,739,307
R^2	0.014	0.064	0.009

Standard errors in parentheses, clustered at 2 digit industry level. Controls are 2 digit SIC industry, 2 digit postcode, and year fixed effects. Years included are 1997-2017. Columns 1 and 2 are linear probability models for the probability of firm birth and death respectively, in the year of the mass layoff. Column 3 is the combined effect on the change in $\log(L)$ including firm birth and death.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.12: Weighted firm level employment regressions

	(1)	(3)	(5)	(6)
Dep. Var	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$	$\Delta\log(L)$
Weighting:	None	L	$\log(L)$	$\log(L)$
Firm birth	No	No	No	Yes
dist 0-1km	-0.0702***	-0.174***	-0.113***	-0.0484***
	(0.00769)	(0.0401)	(0.0166)	(0.0127)
dist 1-2km	-0.0266***	-0.0762***	-0.0462***	-0.00994
	(0.00362)	(0.0177)	(0.00752)	(0.00545)
dist 2-3km	-0.0147***	-0.0513*	-0.0251***	-0.00837
	(0.00305)	(0.0230)	(0.00530)	(0.00522)
dist 3-4km	-0.00813**	0.0153	-0.0124***	-0.000274
	(0.00269)	(0.0179)	(0.00333)	(0.00353)
dist 4-5km	-0.00792**	-0.0221	-0.0130***	-0.00210
	(0.00254)	(0.0176)	(0.00270)	(0.00257)
dist 5-10km	-0.00683***	-0.0125	-0.00778***	0.000131
	(0.00179)	(0.0153)	(0.00221)	(0.00216)
dist 10-20km	-0.00424***	-0.0280	-0.00230	0.00180
	(0.00119)	(0.0221)	(0.00199)	(0.00214)
dist 20-40km	-0.0000507	0.00165	0.00309**	0.00434***
	(0.00121)	(0.00945)	(0.00115)	(0.00114)
Constant	-0.0426***	-0.337***	-0.146***	-0.0825***
	(0.00641)	(0.0688)	(0.00949)	(0.0122)
Controls	Yes	Yes	Yes	Yes
Observations	66,014,522	66,014,522	42,470,811	47,214,972
R^2)	0.023	0.045	0.031	0.016

Standard errors in parentheses, clustered at 2 digit industry level. Controls are 2 digit SIC industry, 2 digit postcode, and year. Years included are 1997-2017. Columns 1,2 and 3 include only firms alive at the start of the period, column 4 also includes firms born during the period.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix C

Appendix for Chapter 3

C.1 O*NET tasks used to construct indices

Sourced from Acemoglu and Autor (2011): O*NET task measures used in this paper are composite measures of O*NET importance scales of work abilities, work activities, work context and skills:

Non-routine cognitive: Analytical

- 4.A.2.a.4 Analyzing data/information
- 4.A.2.b.2 Thinking creatively
- 4.A.4.a.1 Interpreting information for others

Non-routine cognitive: Interpersonal

- 4.A.4.a.4 Establishing and maintaining personal relationships
- 4.A.4.b.4 Guiding, directing and motivating subordinates
- 4.A.4.b.5 Coaching/developing others

Routine cognitive

- 4.C.3.b.7 Importance of repeating the same tasks
- 4.C.3.b.4 Importance of being exact or accurate
- 4.C.3.b.8 Structured v. Unstructured work (reverse)

Routine manual

- 4.C.3.d.3 Pace determined by speed of equipment
- 4.A.3.a.3 Controlling machines and processes
- 4.C.2.d.1.i Spend time making repetitive motions

Non-routine manual physical

- 4.A.3.a.4 Operating vehicles, mechanized devices, or equipment

- 4.C.2.d.1.g Spend time using hands to handle, control or feel objects, tools or controls
- 1.A.2.a.2 Manual dexterity
- 1.A.1.f.1 Spatial orientation

Non-routine manual interpersonal – adapted from Acemoglu and Autor (2011).

- 2.B.1.a Social Perceptiveness
- 4.C.1.a.2.l Face-to-face discussions (Added by current authors)
- 4.A.4.a.5 Assisting and Caring for Others (Added by current authors)

Offshorability

- 4.A.4.a.8 Performing for or Working Directly with the Public (reverse)
- 4.A.4.a.5 Assisting and Caring for Others (reverse)
- 4.C.1.a.2.l Face-to-face discussions (reverse)
- 4.A.1.b.2 Inspecting Equipment, Structures, or Material (reverse)
- 4.A.3.a.2 Handling and Moving Objects (reverse)
- 4.A.3.b.4 0.5*Repairing and Maintaining Mechanical Equipment (reverse)
- 4.A.3.b.5 0.5*Repairing and Maintaining Electronic Equipment (reverse)

C.2 Top 10 of occupations for each skill index, ordered

Non-routine Cognitive Analytical		Non-routine Manual Personal	
212	Mathematicians, actuaries and statisticians	112	Managing Directors and Chief Executives
261	Legal professionals	342	Sports and Fitness Workers
112	Managing Directors and Chief Executives	133	Information and Communications Technology Services Managers
252	Database specialists and systems administrators	122	Sales, Marketing and Development Managers
211	Physicists, chemists and related professionals	143	Other Services Managers
251	Software and applications developers and analysts	134	Professional Services Managers
225	Veterinarians	222	Nursing and Midwifery Professionals
231	University and higher education teachers	233	Secondary Education Teachers
216	Architects, Planners, Surveyors and Designers	322	Nursing and Midwifery Associate Professionals
214	Engineering professionals	132	Manufacturing, Mining, Construction and Distribution Managers
Routine Cognitive		Routine Manual	
523	Cashiers and Ticket Clerks	814	Rubber, Plastic and Paper Products Machine Operators
431	Numerical Clerks	834	Mobile Plant Operators
421	Tellers, Money Collectors and Related Clerks	815	Textile, Fur and Leather Products Machine Operators
422	Client Information Workers	752	Wood Treaters, Cabinet-makers and Related Trades Workers
324	Veterinary Technicians and Assistants	812	Metal Processing and Finishing Plant Operators
251	Software and Applications Developers and Analysts	817	Wood Processing and Papermaking Plant Operators
413	Keyboard Operators	816	Food and Related Products Machine Operators
441	Other Clerical Support Workers	811	Mining and Mineral Processing Plant Operators
541	Protective Services Workers	961	Refuse Workers
821	Assemblers	813	Chemical and Photographic Products Plant and Machine Operators

Non-routine Manual Physical		Non-routine Manual Personal	
833	Heavy Truck and Bus Drivers	322	Nursing and Midwifery Associate Professionals
835	Ships' Deck Crews and Related Workers	342	Sports and Fitness Workers
834	Mobile Plant Operators	531	Child Care Workers and Teachers' Aides
723	Machinery Mechanics and Repairers	514	Hairdressers, Beauticians and Related Workers
931	Mining and Construction Labourers	532	Personal care workers in health services
831	Locomotive Engine Drivers and Related Workers	341	Legal, social and religious associate professionals
741	Electrical Equipment Installers and Repairers	541	Protective services workers
811	Mining and Mineral Processing Plant Operators	325	Other health associate professionals
961	Refuse Workers	511	Travel attendants, conductors and guides
711	Building Frame and Related Trades Workers	516	Other personal services workers
Non-routine Manual Personal (Acemoglu and Autor definition)		**Offshoring**	
233	Secondary Education Teachers	952	Street and related sales and service workers
222	Nursing and Midwifery Professionals	431	Numerical Clerks
322	Nursing and Midwifery Associate Professionals	251	Software and applications developers and analysts
232	Vocational Education Teachers	212	Mathematicians, actuaries and statisticians
342	Sports and Fitness Workers	241	Finance professionals
261	Legal professionals	112	Managing Directors and Chief Executives
235	Other teaching professionals	261	Legal professionals
263	Social and religious professionals	215	Electrotechnology engineers
511	Travel attendants, conductors and guides	264	Authors, journalists and linguists
531	Child Care Workers and Teachers' Aides	331	Financial and mathematical associate professionals

C.3 Additional tables

C.3.1 Age

Table C.1: Baseline regressions with sample stratified along age lines - non-routine cognitive

	(1)	(2)	(3)	(4)
	Hours per worker			
	—- Analytical —-		—– Personal —–	
	Younger	Older	Younger	Older
High Index	2.9047***	4.2682***	3.3435***	4.4218***
	(0.6380)	(0.8704)	(0.5133)	(0.7733)
t	-0.0833***	-0.0324*	-0.0731***	-0.0313**
	(0.0130)	(0.0167)	(0.0118)	(0.0154)
High Index * t	0.0304	0.0187	-0.0215	-0.0105
	(0.0244)	(0.0321)	(0.0263)	(0.0336)
Constant	41.6335***	50.7438***	40.8304***	49.7384***
	(0.6122)	(1.7956)	(0.6120)	(1.8354)
Observations	3741843	2209524	3741843	2209524
R-squared	0.1326	0.1614	0.1294	0.1583
Controls	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Baseline regressions with sample stratified along age lines - routine

	(1)	(2)	(3)	(4)
	Hours per worker			
	—- Cognitive —-		—- Manual —	
	Younger	Older	Younger	Older
High Index	-2.0383***	-2.2006***	2.0963***	3.1891***
	(0.3613)	(0.3745)	(0.3921)	(0.5554)
t	-0.1118***	-0.0547***	-0.0624***	0.0180
	(0.0151)	(0.0144)	(0.0126)	(0.0144)
High Index * t	0.0623***	0.0390**	-0.1003***	-0.2008***
	(0.0175)	(0.0170)	(0.0224)	(0.0309)
Constant	40.7816***	50.1531***	39.4683***	48.6488***
	(0.6457)	(1.9100)	(0.6980)	(1.9206)
Observations	3741843	2209524	3741843	2209524
R-squared	0.1182	0.1428	0.1167	0.1422
Controls	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Baseline regressions with sample stratified along age lines - non-routine manual

	(1)	(2)	(3)	(4)
	Hours per worker			
	—– Physical —–		—- Personal —-	
	Younger	Older	Younger	Older
High Index	2.4243***	2.8672***	-2.3102***	-3.3677***
	(0.4614)	(0.6288)	(0.8280)	(1.1271)
t	-0.0832***	-0.0299*	-0.0904***	-0.0644***
	(0.0128)	(0.0163)	(0.0130)	(0.0131)
High Index * t	-0.0492**	-0.0728***	-0.0035	0.0650
	(0.0208)	(0.0260)	(0.0299)	(0.0407)
Constant	38.7660***	48.1199***	41.3585***	50.8499***
	(0.7630)	(1.9367)	(0.6556)	(2.0225)
Observations	3741843	2209524	3794069	2228632
R-squared	0.1197	0.1430	0.1196	0.1432
Controls	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.3.2 Gender

Table C.4: Baseline regressions with sample stratified along gender lines - non-routine cognitive

	(1)	(2)	(3)	(4)
	Hours per worker			
	—- Analytical —-		—- Personal —-	
	Female	Male	Female	Male
High Index	2.9570***	2.4960***	2.6978***	3.5570***
	(1.0006)	(0.5438)	(0.7407)	(0.5402)
t	-0.0752***	-0.0877***	-0.0827***	-0.0773***
	(0.0206)	(0.0105)	(0.0174)	(0.0106)
High Index * t	0.0542	0.0326	0.0307	0.0020
	(0.0385)	(0.0198)	(0.0309)	(0.0242)
Constant	35.7355***	42.6126***	34.9511***	42.1078***
	(0.9896)	(0.5160)	(1.0092)	(0.5239)
Observations	7800573	8953854	7800573	8953854
R-squared	0.1572	0.1138	0.1532	0.1194
Controls	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Baseline regressions with sample stratified along gender lines - routine

	(1)	(2)	(3)	(4)
	Hours per worker			
	—- Cognitive —-		—- Manual —-	
	Female	Male	Female	Male
High Index	0.1940	-1.7092***	2.8824***	-0.2462
	(0.3842)	(0.2155)	(0.4888)	(0.2425)
t	-0.0710***	-0.0996***	-0.0179	-0.0619***
	(0.0188)	(0.0127)	(0.0149)	(0.0135)
High Index * t	-0.0096	0.0419***	-0.2684***	-0.0515***
	(0.0188)	(0.0119)	(0.0320)	(0.0129)
Constant	34.8137***	42.1432***	34.4231***	42.5968***
	(0.9307)	(0.5634)	(0.9209)	(0.5379)
Observations	7800573	8953854	7800573	8953854
R-squared	0.1415	0.1016	0.1465	0.1012
Controls	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Baseline regressions with sample stratified along gender lines - non-routine manual

	(1)	(2)	(3)	(4)
	Hours per worker			
	—- Physical —-		—- Personal —-	
	Female	Male	Female	Male
High Index	1.9800***	-0.3299	-0.8496	-1.3211
	(0.4901)	(0.2741)	(0.7523)	(0.9165)
t	-0.0616***	-0.0732***	-0.0802***	-0.0869***
	(0.0159)	(0.0147)	(0.0158)	(0.0123)
High Index * t	-0.1089***	-0.0267*	0.0049	-0.0122
	(0.0314)	(0.0141)	(0.0296)	(0.0236)
Constant	34.3051***	42.6849***	35.2386***	42.2921***
	(0.9264)	(0.5703)	(0.7979)	(0.5367)
Observations	7800573	8953854	7846974	9112745
R-squared	0.1420	0.1001	0.1422	0.0987
Controls	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.3.3 Education

Table C.7: Baseline regressions with sample stratified along education - non-routine cognitive

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	——— Analytical ———			——— Personal ———		
	Low	Mid	High	Low	Mid	High
High Index	4.9619***	2.8143***	2.8265***	4.6360***	4.0738***	3.3599***
	(1.1206)	(0.6866)	(0.6825)	(0.5753)	(0.7001)	(0.6549)
t	-0.0939***	-0.0849***	-0.0228	-0.1000***	-0.0789***	-0.0016
	(0.0160)	(0.0112)	(0.0176)	(0.0148)	(0.0115)	(0.0117)
High Index * t	0.0065	0.0244	0.0229	-0.0095	-0.0497	-0.0024
	(0.0467)	(0.0250)	(0.0239)	(0.0252)	(0.0322)	(0.0283)
Constant	39.3707***	44.6373***	43.6233***	39.1210***	44.0347***	42.7427***
	(0.5843)	(0.4598)	(0.9699)	(0.5668)	(0.4626)	(0.8291)
Observations	4945745	7384674	4424008	4945745	7384674	4424008
R-squared	0.2629	0.2107	0.1815	0.2620	0.2116	0.1820
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector FEs	Yes	Yes	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Baseline regressions with sample stratified along education - routine

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	——- Cognitive ——-			——- Manual ——-		
	Low	Mid	High	Low	Mid	High
High Index	-0.4515	-1.2496***	-2.2887***	0.9152**	-0.1222	-0.6108
	(0.3016)	(0.2867)	(0.5821)	(0.3760)	(0.2943)	(0.4560)
t	-0.1278***	-0.1185***	-0.0218	-0.0372**	-0.0653***	-0.0140
	(0.0139)	(0.0144)	(0.0238)	(0.0163)	(0.0141)	(0.0172)
High Index * t	0.0677***	0.0601***	-0.0102	-0.1185***	-0.0634***	-0.1003***
	(0.0167)	(0.0146)	(0.0280)	(0.0238)	(0.0188)	(0.0235)
Constant	40.3039***	44.8883***	42.9483***	39.4692***	45.0335***	44.2822***
	(0.5724)	(0.4366)	(1.0153)	(0.6274)	(0.4210)	(0.9760)
Observations	4945745	7384674	4424008	4945745	7384674	4424008
R-squared	0.2473	0.1975	0.1742	0.2484	0.1990	0.1684
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector FEs	Yes	Yes	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.9: Baseline regressions with sample stratified along education - non-routine manual

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours per worker					
	——- Physical ——-			——- Personal ——-		
	Low	Mid	High	Low	Mid	High
High Index	-0.0638	-0.4136	-0.3730	-0.7455	-1.3270	-2.2299***
	(0.3555)	(0.3428)	(0.4946)	(1.2181)	(0.8603)	(0.6462)
t	-0.1168***	-0.0916***	-0.0160	-0.1180***	-0.1019***	-0.0250
	(0.0164)	(0.0132)	(0.0174)	(0.0130)	(0.0151)	(0.0184)
High Index * t	0.0187	0.0021	-0.0698***	0.0697	0.0287	-0.0422
	(0.0216)	(0.0171)	(0.0232)	(0.0430)	(0.0270)	(0.0272)
Constant	39.8746***	44.9661***	44.3864***	40.1727***	45.0066***	43.5932***
	(0.6365)	(0.4649)	(0.9950)	(0.5512)	(0.4394)	(1.0307)
Observations	4945745	7384674	4424008	4976773	7463390	4519556
R-squared	0.2468	0.1972	0.1667	0.2463	0.1972	0.1719
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector FEs	Yes	Yes	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.4 Non-routine manual personal Definition of Acemoglu and Autor (2011)

Table C.10: Baseline

	(1)	(2)	(3)	(4)
	Hours per worker			
High Index	-3.1303***	-0.9984**	-0.5236	-0.5120
	(0.5991)	(0.4760)	(0.4660)	(0.4615)
t	-0.1036***	-0.0850***	-0.0860***	-0.0878***
	(0.0130)	(0.0133)	(0.0112)	(0.0108)
High Index*t	-0.0171	0.0140	0.0411**	0.0267
	(0.0267)	(0.0225)	(0.0208)	(0.0198)
Constant	39.3242***	39.3638***	42.3381***	41.3666***
	(0.4231)	(1.1248)	(0.7431)	(0.5124)
Observations	21831786	16959719	16959719	16959719
R-squared	0.0132	0.1462	0.1705	0.1932
Controls	No	Yes	Yes	Yes
Country FE	No	Yes	Yes	No
Sector FE	No	No	Yes	No
Country-Sector FE	No	No	No	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

0.9* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.11: Age and gender

	(1)	(2)	(3)	(4)
	Hours per worker			
	——Age——		——Gender——	
	younger	older	female	male
High Index	-1.0136**	-1.6268**	-0.7760	0.1131
	(0.4987)	(0.6999)	(0.6304)	(0.3008)
t	-0.0910***	-0.0581***	-0.0863***	-0.0865***
	(0.0120)	(0.0114)	(0.0149)	(0.0120)
High Index * t	0.0035	0.0477	0.0510*	-0.0299*
	(0.0205)	(0.0332)	(0.0268)	(0.0170)
Constant	40.8567***	50.2047***	35.0262***	42.1684***
	(0.6234)	(1.9759)	(0.8779)	(0.5394)
Observations	3794069	2228632	7846974	9112745
R-squared	0.1140	0.1396	0.1416	0.0971
Controls	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.12: Education and full-time/part-time status

	(1)	(2)	(3)	(4)	(5)
	Hours per worker				
	——-Education——-			——Status——	
	low	mid	high	FT	PT
High Index	0.1550	-0.4186	-1.0393*	-0.3378	0.0239
	(0.5415)	(0.3101)	(0.5302)	(0.2438)	(0.4780)
t	-0.1145***	-0.1026***	-0.0248	-0.0378***	0.0189
	(0.0126)	(0.0125)	(0.0182)	(0.0108)	(0.0120)
High Index * t	0.1034***	0.0644***	-0.0634**	-0.0088	0.0380
	(0.0316)	(0.0217)	(0.0260)	(0.0127)	(0.0274)
Constant	40.0030***	44.7952***	43.4015***	53.1620***	10.9220***
	(0.5262)	(0.4017)	(1.0088)	(0.5503)	(0.8640)
Observations	4976773	7463390	4519556	13555103	3356901
R-squared	0.2474	0.1967	0.1683	0.1465	0.1369
Controls	Yes	Yes	Yes	Yes	Yes
Country-Sector FE	Yes	Yes	Yes	Yes	Yes

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1992-2016.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.5 Country-level results

Table C.13: Country results - coefficient on interaction term only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	NRCA	NRCP	RC	RM	NRMP	NRMI	Off
AT	0.0511***	−0.0303***	0.0360***	0.0061*	0.0733***	−0.0560***	−0.0699***
BE	0.0867***	0.0825***	−0.0355***	−0.0811***	−0.0146***	−0.0535***	−0.0631***
DE	0.0051	−0.0686***	0.0465***	−0.1990***	−0.0538***	0.0713***	−0.1150***
DK	−0.0627***	−0.0344***	−0.0217***	−0.0947***	0.0248***	0.0417***	−0.1120***
ES	0.1070***	0.0097***	0.0136***	−0.0520***	0.0250***	−0.1010***	−0.0401***
FI	0.0005	−0.0791***	0.0390***	−0.0166**	0.0411***	−0.0129*	−0.0366***
FR	0.1020***	0.1020***	−0.0320***	−0.1390***	−0.0383***	−0.0062*	−0.0654***
GR	0.0569***	0.0257***	0.0636***	−0.0397***	−0.0292***	0.0031	0.0222***
IE	0.1120***	0.0238***	0.0259***	−0.0658***	−0.0420***	−0.1360***	0.0979***
IT	0.1660***	0.1080***	0.0390***	−0.1020***	−0.0143***	−0.0447***	−0.0417***
LU	0.0072	−0.0358***	−0.0061	−0.0099*	0.0292***	−0.0879***	0.0172***
NL	0.0306***	−0.0416***	−0.0464***	−0.1650***	−0.1150***	0.0242***	0.0033
PT	0.1330***	0.1020***	0.0520***	−0.1430***	−0.0712***	0.0405***	0.0227***
SE	−0.0282***	−0.1010***	−0.0110***	−0.0823***	−0.0085***	0.0038	−0.0552***
UK	−0.1090***	−0.0763***	0.0088***	−0.1750***	−0.1130***	0.0972***	−0.0450***

All regressions weighted with EU-LFS weights, and standard errors clustered at country-sector level. Demographic controls: age, educational level, sex, size of firm, proxy interview, marital status. Industry controls are 1 digit NACE. The coefficient is from the interaction term of the High Index with the trend. High Index is a dummy that takes value 1 if the occupation is above the 66^{th} percentile for the index. Sample is individuals working non-zero hours in EU-15 countries from 1998-2016.

NRCA=Non-routine cognitive analytical, NRCP=Non-routine cognitive personal, RC=Routine cognitive, RM=Routine manual, NRMP=Non-routine manual - physical, NRMI=Non-routine manual - personal, Off=Offshorability

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$